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EXHIBITING

A CONCISE VIEW OF THE LATEST AND MOST
IMPORTANT DISCOVERIES IN MEDICINE
AND MEDICAL PHILOSOPHY.

COLLECTED AND PUBLISHED

BY

ANDREW DUNCAN, M. D. F. R. & A. S. Ed.

PHYSICIAN TO HIS ROYAL HIGHNESS THE PRINCE OF WALES
FOR SCOTLAND,
FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH,
AND MEMBER OF THE ROYAL SOCIETIES OF MEDICINE
OF PARIS, COPENHAGEN, EDINBURGH, *&c.*

> *Neglecta reducit, ſparſa colligit, utilia ſelegit, neceſſaria oſtendit, ſic utile.* BAGLIVIUS.

VOLUME TENTH.

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TO

SIR THOMAS DUNDAS
OF KERSE, BARONET,

THIS VOLUME

IS RESPECTFULLY INSCRIBED,

IN TESTIMONY OF ESTEEM,

FOR HIS PUBLIC CONDUCT,

AND PRIVATE VIRTUES,

BY HIS

MOST OBEDIENT SERVANT,

ANDREW DUNCAN.

——— Ille potens ſui
Lætuſque deget, cui licet in diem
Dixiſſe vixi.——— HORATIUS.

Hist. of Science

PREFACE.

IT has ever been the object of theſe Commentaries to collect uſeful information from different ſources. And I flatter myſelf with the hope, that, in this reſpect, the preſent volume will not be deemed inferior to former ones. Beſides an abridged view of important diſcoveries, extracted from the tranſactions of public ſocieties and the writings of individuals, I have been enabled to preſent the reader with valuable original obſervations, not merely from eminent practitioners in Britain and Ireland, but from ſome alſo in very diſtant countries; intereſting communications having been tranſmitted to me from the Weſt and Eaſt Indies, from Ruſſia, and from Hudſon's Bay.

To this tenth volume, I have ſubjoined a general alphabetical table of contents, for it and the four preceding volumes. This table, in a work now extended through ſeveral volumes, each conſiſting of a number of detached obſervations, cannot fail to be uſeful to thoſe who may have occaſion to conſult it on particular ſubjects; and while it is executed upon a plan more ſimple than that which was annexed to the fifth volume, I flatter myſelf it will alſo be found more convenient.

With the eleventh volume, I am to begin as it were a new ſeries. It is not, however, my intention to alter the plan of the work. I ſtill propoſe to continue it, as far as circumſtances will permit, under the form of an annual volume. This, however, will not ſo much depend upon myſelf, as upon the communications I receive from others. But after the aſſiſtance with which I have already been ſupported, I truſt there will not be wanting many medical practitioners who, by conveying uſeful information to the public, through the channel of this work, will thereby confer an obligation on

Their moſt obedient ſervant,

ANDREW DUNCAN.

EDINBURGH,
Jan. 1. 1786.

CONTENTS

OF

VOLUME TENTH.

SECT. I. Account of New Books.

Page.

I. Spallanzani, Abbé, *Diſſertations relative to the natural hiſtory of animals and vegetables, tranſlated from the Italian,* 1

II. Sauſſure, Horace Benedict de, *Eſſais ſur l'Hygrometrie,* 36

III. Bergmann, Sir Torbern, *Phyſical and Chemical Eſſays, tranſlated from the original Latin, by* Edmund Cullen, M. D. 56

IV. Michell, Jani Peterſen, M. D. *De ſynchondrotomia pubis Commentarius,* 92

V. Lavoiſier, M. *Expériences ſur la reſpiration des animaux, et ſur les changemens qui arrivent à l'air en paſſant par leur poumon,* 97

VI. Bordenave, M. *Mémoire ſur la néceſſité de faire l'operation Ceſarienne aux femmes qui*

Page.

qui meurent encientes, et ſur les moyens de rapeller leurs enfans d'une mort apparente à la vie, 102

VII. Milly, M. le Comte de, *Mémoire ſur une ſubſtance aëriforme qui émane du corps humain, et ſur la maniere de la recueillir,* 108

VIII. Portal, M. *Mémoire ſur quelques maladies du foie qu'on attribue à d'autres organes, et ſur des maladies dont ou fixe ordinairement le ſiége dans le foie, quoiqu'il n'y ſoit pas,* 115

IX. Fowler, Thomas, M. D. *Medical Reports of the effects of tobacco, principally with regard to its diuretic quality in the cure of dropſies and dyſuries: together with ſome obſervations on the uſe of glyſters of tobacco, in the treatment of the colic,* 122

X. Withering, William, *Account of the fox-glove, and ſome of its medicinal uſes; with practical remarks on dropſies, and other diſeaſes,* 133

XI. Thueſſink, Everardi Joannis Thomaſſen, *A. L. M. Philoſ. Doct. Soc. Reg. Med. Edn. et Hiſt. Nat. Stud. Ed. Sod. nec non Muſæi Pariſini, ac Soc. Phyſico-Med. Hagan.*

Page.
a Comm. Literario, Diſſertatio de opii uſu in ſiphylide, obſervatis probato, 146
XII. Hope, John, M. D. F. R. S. *Deſcription of a plant yielding aſafœtida, in a letter to* Sir Joſeph Banks, Bart. P. R. S. 160
XIII. Monro, Alexander, M. D. *The ſtructure and phyſiology of fiſhes explained, and compared with thoſe of man, and other animals,* 165
XIV. Gardiner, John, M. D. *Obſervations on the animal œconomy, and on the cauſes and cure of diſeaſes,* 188

SECT. II. MEDICAL OBSERVATIONS.

I. *Obſervations on droſipes prevailing among the troops in the Eaſt Indies. By* Mr William Dick, *ſurgeon of artillery, Bengal eſtabliſhment, in a letter to Dr* Duncan, *from Madras,* 207
II. *Some remarks on certain articles of the materia medica, communicated in a letter to Dr* Duncan, *from Antigua, by Dr* James Adair, *now phyſician in Bath,* 233
III. *The*

Page.
III. *The hiſtory of a caſe of catalepſis, ſucceſsfully treated by Dr* J. Fitzpatrick *of Dublin*, 242
IV. *Account of the good effects obtained from the calx of zinc, in a hyſterical affection. Communicated to Dr* Duncan, *in a letter from Glaſgow, by Dr* Alexander Maclachlan, 247
V. *Account of an uncommon caſe in midwifery, where a preternatural adheſion of twins had taken place. By Dr* James Oliphant, *Phyſician at Irvine, communicated to Dr* Monro, 249
VI. *Hiſtory of two caſes of painful conſtipation from indurated fæces, with remarks on that affection. By Dr* John Warren, *phyſician at Taunton, communicated in a letter to Dr* Duncan, 255
VII. *Obſervations on the Peruvian bark. By* T. Colingwood, *ſurgeon, Alnwick.* 265
VIII. *The hiſtory of a caſe in which a large wound of the abdomen, with a remarkable protruſion of the inteſtines, terminated favourably. By Dr* Thomas Cochrane, *phyſician in St Chriſtopher's*, 276

IX. *Remarks*

Page.

IX. *Remarks on the Sigualtian operation, extracted from a letter to Dr* Duncan. *By Dr* J. H. Myers *of London, written from Paris,* 281

X. *Obſervations on the uſe of the cuprum ammoniacum, in the cure of the chorea Sancti Viti. Communicated to Dr* Duncan, *by* Joſhua Walker, *M. D. Phyſician to the Leeds Infirmary,* 288

XI. *The hiſtory of a caſe of ileus, terminating fatally, with an account of the appearances on diſſection. By Dr* James Gerard, *phyſician in Liverpool,* 293

XII. *The hiſtory of a caſe of hydrocephalus, terminating ſucceſsfully. By Dr* John Evans, *phyſician in Liverpool,* 299

XIII. *The hiſtory of an uncommon ſwelling in the lower extremities of a pregnant woman, terminating favourably immediately after an abortion. By Dr* John Evans, *phyſician at Liverpool,* 302

XIV. *Obſervations on the gaſtric juice. By* * * * *phyſician in London,* 305

Page.
XV. *Account of appearances on the diſſection of a child dying of hydrocephalus. By Dr* Joſhua Dixon, *phyſician, Whitehaven,* 312

SECT. III. MEDICAL NEWS.

1. *Account of the intended eſtabliſhment of new medical profeſſorſhips in the univerſity of Dublin,* 318
2. *Account of the late Dr* John Parſons *of Oxford,* 322
3. *Account of the late Dr* Richard Huck-Saunders *of London,* 325
4. *Account of the late Dr* Charles Colignon *of Cambridge,* 332
5. *Account of the late* Sir Alexander Dick *of Preſtonfield,* 334
6. *Account of prizes by the Harveian Society of Edinburgh,* 340
7. ———— *by the Medical Society of Edinburgh,* 347
8. ———— *by the Faculty of Medicine of Goettingen,* 349
9. ———— *by the Academy of Sciences of Berlin,* 350

10. *Account*

Page.
10. *Account of prizes by the Academy of Sciences of Paris,* 351
11. *Abſtract from a meteorological regiſter kept in the neighbourhood of Edinburgh,* 353
12. *Account of the cultivation of cloves and nutmegs in the Iſle of France,* 354
13. *Account of the ſucceſs of mercury in a caſe of hydrocephalus,* 356
14. *Account of the effects of foxglove in caſes of inſanity, hæmoptyſis, and dropſy,* 357
15. *Account of the effects of opium in ſyphilis, in the hoſpital at Liſle,* 361
16. *Account of effects from tranſplanting teeth,* 362
17. *Account of experiments for determining the effects which bathing in Buxton water has on the perſpiration,* 363
18. *Account of the cold produced at Hudſon's Bay, by the mixture of vitriolic acid and ſnow,* 367
19. *Account of experiments reſpecting the freezing of mercury,* 368
20. *Account of the good effects of mercury in a caſe of rabies,* 369
21. *Account of a valuable Ruſſian malachite,* 373
22. *Account of intended publications,* 376

23. *Account*

Page.
23. *Account of prizes by the Medical Society of Paris,* 382
24. ———— *by the Royal Society of London,* 387
25. *Account of medical promotions at Oxford, Cambridge, and Edinburgh,* 389

SECT. IV. List of New Books, 396

MEDI-

MEDICAL

COMMENTARIES.

SECT. I.

Account of New Books.

I.

Diſſertations relative to the Natural Hiſtory of Animals and Vegetables, tranſlated from the Italian of the Abbé Spallanzani, *Royal Profeſſor of Natural Hiſtory, in the Univerſity of Pavia, Superintendant of the Public Muſeum, and Fellow of various learned Societies.* 2 vols. 8vo, London.

THE name of Spallanzani has been conſidered as juſtly entitled to an eminent ſtation among the philoſophers of the preſent age. Among natural hiſtorians he has acquired a very high degree of reputation, both for the accuracy and ingenuity of his experiments, and for the importance as well as novelty of his concluſions. The work now before us, in which many diſcoveries equally ſingular and intereſting are contained, was firſt publiſhed by the author in the Italian language, and is now preſented to the Britiſh reader in an Engliſh dreſs. To the preſent tranſlation, which is entitled to a very high degree of merit, for its accuracy as well as its elegance, are added ſome important diſcoveries by philoſophers of our own Iſland; particularly the experiments and obſervations of the ingenious Mr John Hunter on digeſtion, and thoſe of Mr Debraw on the fecundation of bees, extracted from the Philoſophical Tranſactions. Theſe may juſtly be conſidered as no unintereſting addition to the preſent work. But as we have already given ſome account of theſe papers in former volumes of our Commentaries, we ſhall here confine our analyſis to the diſſertations of Spallanzani himſelf.

The firſt ſubject, which, in the work before us, engages the attention of our author, is the important function of digeſtion. After a ſhort introduction, pointing out the circumſtances which firſt led him to turn his attention to the action of muſcular, intermediate, and what he calls membranous ſtomachs, he proceeds, in his firſt diſſertation, to treat of the digeſtion of animals with muſcular ſtomachs; ſuch as common fowls, turkeys, ducks, geeſe, pigeons, and the like.

He ſets out with obſerving, that there perhaps exiſts no animal, the ſtomach of which is not furniſhed with muſcular fibres; but that there is a ſingular claſs juſtly denominated by naturaliſts, animals with muſcular ſtomachs, in whom that viſcus is provided with very large and powerful muſcles. In theſe it was natural to conclude, that digeſtion was performed by the trituration of the aliment; and, from analogy, this was ſuppoſed to be the cauſe of digeſtion with all other animals. The ingenious Mr Reaumur, however, was perhaps one of the firſt who attempted to determine this by accurate experiment. He incloſed in metallic tubes, perforated at both ends, grains of barley, and afterwards obliged common fowls, turkeys, and ducks, to ſwallow thoſe tubes. Several hours afterwards, when the animals were killed, and the tubes taken out of their ſtomachs, the barley was found quite entire. This experiment was very much in favour of the influence of the muſcular action.

But as the experiment had been made with grains of one kind only, our author reſolved to repeat it with different kinds, as wheat, maize and rye. After they had remained twenty-four hours in the ſtomach, they were taken out, and their contents, on examination, appeared to be unaltered. Even the colour and taſte were unchanged, excepting a ſlight bitter flavour which they had acquired. They had indeed imbibed a fluid, and were a little ſwoln; but when they were incloſed in tubes, and left in the ſtomach even for two or three days, they underwent no greater change. And if, at the ſame time with the tubes, looſe grains were introduced, theſe were broken down in a few hours.

As the food taken ſpontaneouſly by theſe animals, ſtops ſome time in the crop, where it is macerated and becomes ſofter, Abbé Spallanzani repeated his experiments with ſeeds taken from the crop of a fowl, after they had there undergone a complete maceration. But notwithſtanding this preparation, they underwent no change within the tubes. Beſides, varying theſe experiments in different ways, he repeated them, by employing, in place of metallic tubes, glaſs ſpherules, perforated with a number of holes, and admitting more free acceſs to fluids. As a greater quantity of liquor could now find its way to the incloſed ſubſtances, they acquired a more bitter taſte, but he could never perceive the ſlighteſt token of ſolution, though they continued in the ſtomach for a long time.

Theſe facts, he thinks, furniſh irrefragable proof, that the trituration of ſeeds in the ſtomach of granivorous animals is ſolely owing to ſtrong preſſure, and to repeated and violent percuſſions, effects produced by the powerful muſcles with which that organ is provided.

In the conduct of theſe experiments, our author points out ſeveral circumſtances which deſerve attention, particularly thoſe which ariſe from the contents of the ſtomach being driven in at the apertures left for the admiſſion of the gaſtric juice, and thoſe which ariſe from the tubes being deſtroyed, or otherwiſe injured by the action of the ſtomach. The firſt is beſt obviated by the tubes being ſwallowed when the ſtomach is empty, and the laſt by their being of conſiderable ſtrength. And here he takes occaſion to mention the wonderful effects which the action of the ſtomach has in breaking and diſtorting tin tubes, even when ſtrengthened by iron wire. In the courſe of theſe experiments alſo, he received full confirmation of the Florentine experiments, ſhewing the trituration of empty glaſs globules in the ſtomach of gallinaceous fowls. He found, that the longer they remained in the ſtomach, the more finely were they pulverized. And that the facility with which they were broken was, in general, in proportion to the ſize of the animal. Thus a pigeon generally breaks them leſs ſpeedily than a chicken, a chicken than a capon, and a gooſe ſooner than any of them. This he, with great probability, aſcribes to the larger ſpecies having thicker and more powerful ſtomachs.

On this ſubject, it was an object of curioſity to enquire, what would happen when ſharp bodies were introduced into the ſtomach. He found that pieces of glaſs loſt their edges and points by continuing twenty-four hours in the gizzard of a cock. The glaſs loſt conſiderably of its weight; nor was it difficult to conceive what became of the miſſing particles, for the ſides of the ſtomach glittered with innumerable vitreous points. This appeared to be entirely the effect of muſcular action, for broken bits of glaſs, incloſed in tubes, were in no degree abraded, either at their points or edges. Among other experiments of this kind, he gave to a wood-pigeon an unpoliſhed twelve-ſided garnet, of the ſize of a moderate nut. This garnet, at the end of a month, was found to fill almoſt the whole capacity of its ſtomach, although it had taken its food, and been nouriſhed very well. But what was ſtill more ſurpriſing, the angles of this hard ſtone were blunted in many places. From the moſt accurate examination, it appeared, that the coats of the ſtomach were in no degree hurt by the glaſs, or other hard ſubſtances. But the coat of that ſtomach which had retained the garnet for a month, was about three times as thick as it commonly is.

Finding that the ſtomach received no injury in theſe experiments, Abbé Spallanzani fixed needles and lancets, with their points projecting, into leaden balls, and forced them into the ſtomachs of turkeys. The points of theſe he found were broken off, by the action of the ſtomach, without any injury to the organ itſelf. He found, however, that the ſtomachs of young fowls were ſometimes hurt by metallic points, as when they were made to ſwallow pins with the heads taken off.

It has been imagined by ſome, that the ſtomachs of fowls may be defended by theſe ſmall peebles which are conſtantly found in them, and which, on accurate examination, our author obſerves are contained even in the gizzards of neſtlings. But by carefully rearing fowls from the time they were hatched, he was able to obtain them without any ſtones in their ſtomachs. And in theſe caſes alſo, this organ deſtroyed ſharp ſubſtances, without ſuſtaining any injury. It appears, therefore, that they are not neceſſary to the trituration of the firmeſt food, or hardeſt foreign ſubſtance, but that this trituration is the immediate effect of the gaſtric muſcles.

Although, from the facts that have been mentioned, incontrovertible proof was afforded, that the food of fowls muſt undergo the mechanical action of the gaſtric muſcles, before it can be broken down, yet, in the opinion of our author, it did not follow, that ſimple trituration converted the aliment into that pultaceous maſs which has been denominated *chyme*. To determine this point, he had again recourſe to the tubes and ſpherules formerly mentioned. Theſe he filled with maſticated bread, with bruiſed fleſh, and other matters not ſoluble by ſimple maceration. And from the ſolution of theſe which took place after being reduced to this condition, deciſive proof was afforded that a ſolution was effected by means of the gaſtric liquor. This, therefore, he conſidered as the cauſe of digeſtion in theſe caſes.

From combining theſe facts, Abbé Spallanzani concludes, that, in the gallinaceous claſs, trituration and the gaſtric fluid mutually aſſiſt each other in performing the great function of digeſtion. The former, by breaking down the aliment, acts as the prediſpoſing cauſe; the latter, when it is thus prepared, penetrates into it, deſtroys its texture, diſſolves the particles, and diſpoſes them to change their nature, and to become animalized.

Having thus aſcertained the influence of the gaſtric fluid, a queſtion naturally aroſe with reſpect to its origin. For this purpoſe, Abbé Spallanzani attentively examined the organs of digeſtion in different fowls. In a gooſe, he could plainly diſcover the whole œſophagus covered with points, or elongated ſpots; and, from examining theſe by the help of a glaſs, he could clearly perceive that they were follicular glands. And from ſmall excretory ducts, he found that the liquor of theſe follicles might readily be forced out by preſſure. This liquor he found to be tranſparent, ſomewhat viſcid, and its taſte rather ſweet. But no ſuch follicular glands were diſcoverable in the gizzard, which nature has ſagaciouſly inveſted with a cartilaginous coat more capable of reſiſtance.

The ſame appearances as were diſcovered in the gooſe, were alſo detected in the turkey, duck, common fowl, and various ſmaller birds, as the pigeon, partridge, turtle-dove, *&c.* And in theſe fowls provided with a crop or craw, follicular glands were alſo detected in that organ. In all fowls, a fleſhy facia is diſcovered at the lower part of the œſophagus, provided with follicles much larger than thoſe of the crop or œſophagus, and in great abundance. From theſe ſources, the fluid is furniſhed in very conſiderable quantity. This was demonſtrated by the introduction of a ſmall piece of dried ſponge, previouſly cleaned from every impurity, into the craw of a pigeon, in which it was left twelve hours. At the end of that time, the ſponge, when taken out and ſqueezed over a glaſs, afforded an ounce of liquor. From larger fowls, greater quantities were obtained in proportion: Thus, from the craw of a turkey, ſeven ounces of this liquor were obtained in ten hours.

From trials made with this œſophagal or gaſtric fluid, and from contraſting it with water, he found that it poſſeſſed a very conſiderable degree of ſolvent power. But, in order to obtain, by means of it, the ſolution of vegetable and animal ſubſtances, it ſhould be freſh; for he found that it loſt its efficacy when it had been kept for ſome time in veſſels, eſpecially if they happened to be open. It became inefficacious alſo after it had been uſed for an experiment. And, laſtly, he found that a conſiderable degree of heat, equal to the temperature of man or birds, muſt be applied, for without this the gaſtric juices are not more effectual in diſſolving fleſh, or vegetables, than common water.

In the ſecond diſſertation, Abbé Spallanzani treats of the digeſtion of the food in theſe animals who have what are called *intermediate ſtomachs.* By intermediate ſtomachs, he means thoſe which are not, on the one hand, provided with thick and ſtrong ſides, as in the gallinaceous family, and, on the other, are not merely membranous, as in birds of prey and men, but which have an intermediate degree of thickneſs and ſtrength. Of this kind are the ſtomachs of ravens, crows, herons, and many other birds. Theſe birds, as well as man, may be denominated omnivorous. Herbs, graſs, leguminous ſeeds, fleſh of every kind, alive or dead, ſerve equally for their nouriſhment.

Beans and wheat, in their entire ſtate, after remaining forty-eight hours in the ſtomachs of crows, had ſuffered no other alteration but that of being a little moiſtened. The gaſtric fluid, therefore, is incapable of effecting the ſolution of theſe vegetable matters. But theſe articles, when bruiſed, or reduced to the ſtate of a coarſe fluor, were completely diſſolved in twenty-four ty-four hours. Apples, and the crumbs of bread, were diſſolved in a ſtill ſhorter period.

From vegetable, our author proceeded to animal ſubſtances. And here the diſpoſition which they have to frequent vomiting, or rather the privilege which they poſſeſs, of returning indigeſtible matters by the mouth, in the ſame manner as birds of prey diſcharge the feathers and hairs of the animals they have devoured, afforded him an opportunity of marking the gradual progreſs of the ſolution. Eight tubes, containing fleſh not minutely bruiſed, but each tube containing an entire piece, were introduced into the ſtomachs of different crows. In one of theſe, diſcharged at the end of an hour, the fleſh did not appear to be ſenſibly diminiſhed. After two hours and a half, the ſolution had made a ſenſible progreſs; a dark covering of jelly ſurrounded the fleſh, which came away when touched with the finger. In four hours, the fleſh did not amount to half its original quantity; and the tubes vomited at the end of ſeven hours, were found to be perfectly empty. And it deſerves to be remarked, that not the ſmalleſt token of putrefaction could be diſcovered, either during the progreſs, or at the completion of the ſolution. When tubes with larger perforations were employed, ſo as to give freer acceſs to the gaſtric juice, the ſolution was much more rapid, a complete ſolution being effected in leſs than four hours. When the perforations in the tubes were covered with linen, to prevent ſo free an acceſs of the gaſtric juice, the ſolution was retarded in proportion to the degree of covering.

Young crows eat more than adults, and have a quicker digeſtion; and their ſtomachs were, upon examination, found to contain a greater quantity of gaſtric juice. From experiment, it was found that bones are indigeſtible by crows, excepting ſuch as, on account of their ſoftneſs, are rather to be conſidered as cartilage than bone.

After ſome remarks on the anatomical ſtructure of the ſtomach of the crow, the author next proceeds to enquire, whether the œſophagus of this bird will diſſolve fleſh, in the ſame manner as ſolution is performed by the œſophagus of ſome fiſhes. With this view he introduced fleſh faſtened to wires, that it might deſcend only to a limited extent. Although the reſult of his experiments does not permit him to refuſe the œſophagus all the power of digeſtion, yet he found it inconſiderable, when compared with that of the ſtomach. In gallinaceous fowls, he found that no ſolution took place in the craw. Hence he concludes, that the œſophagal liquor in gallinaceous fowls is different from that of crows.

By introducing bits of dry ſponge into the ſtomachs of crows, Abbé Spallanzani obtained the gaſtric fluid in very great purity, without killing the animal. It was of a tranſparent yellow colour, depoſiting very little ſediment; it was of a bitter and ſalt taſte, and had little volatility or inflammability; for it not only extinguiſhed fire, but paper ſoaked in it, did not burn till it was evaporated. It ſeems to be conſtantly ſecreted, and in very conſiderable quantity, inſomuch, that in a few days he was able, from five crows, to collect thirteen ounces of it. In the ſame manner, he obtained alſo the œſophagal liquor; but this he found to be nearly inſipid and colourleſs, while it was ſeparated in much leſs quantity. Hence he concludes, that food is digeſted more ſpeedily and perfectly in the ſtomach, than in the œſophagus, not only from the greater quantity, but alſo from the greater energy, of the gaſtric liquor.

Having obtained a conſiderable quantity of the gaſtric fluid of the crow, he reſolved to try its ſolvent powers out of the body. For greater certainty in theſe experiments, he eſtabliſhed a term of compariſon, by employing ſimilar veſſels containing the ſame fleſh, infuſed in water. In a heat of forty-two degrees of Fahrenheit's thermometer, the gaſtric fluid ſeemed to have no greater ſolvent power than water. The only difference was, that the fleſh immerſed in the former, was preſerved from putrefaction, but not in the latter. But in a heat of ſeventy-nine degrees, the ſolvent power of the gaſtric liquor was very conſiderable, and without the leaſt mark of putrefaction, which was always the caſe when ſolution took place with the water.

To the experiments on crows are added ſeveral on the heron, an animal alſo of an intermediate ſtomach. From theſe experiments it appeared, that the ſtomach of the heron acts with ſome force upon the ſubſtances it contains; but that digeſtion in this animal is not the effect of trituration, but of the gaſtric fluid, and that the efficacy of the gaſtric fluid is not limited to ſoft parts, but extends alſo to bones.

From ſeveral experiments, it was incontrovertibly proved, that the œſophagus of the heron, as well as of the crow, has the power of digeſting any food that may happen to be lodged in it; but that the concoction of the ſtomach is exceſſively rapid, when compared with that of the œſophagus.

Our author concludes this ſecond diſſertation with a compariſon between birds with muſcular, and thoſe with intermediate ſtomachs. And, here, he firſt points out the circumſtances in which there is a reſemblance, and next, thoſe in which they diſagree. With reſpect to reſemblance, he remarks, that their gaſtric fluids, beſides being alike in colour, are always ſalt and bitter, the latter quality probably ariſing from an impregnation of bile; that theſe fluids are the immediate agents of digeſtion, independently of trituration; that the fluids act in the ſame manner in the ſolution of the food, firſt ſoftening and then converting the ſurface into a gelly, and thus gradually inſinuating themſelves till the whole be diſſolved; that they do not loſe their ſolvent power when taken out

of

of the body, provided they be heated to a proper degree ; and laſtly, that in both claſſes of animals, theſe fluids ſpring from follicular glands, with which their organs of digeſtion abound. The principal differences which he points out, are the inferior efficacy of the gaſtric fluid in muſcular, to that of the ſame fluid in intermediate ſtomachs ; and the prodigious effects of trituration in muſcular ſtomachs, with which the feeble force of intermediate ſtomachs is ſcarcely comparable.

After having inſtituted theſe experiments on animals with muſcular and with intermediate ſtomachs, Abbé Spallanzani next proceeds to treat of digeſtion in a much more numerous claſs of animals, thoſe, *viz.* with membranous ſtomachs. In this third diſſertation, he treats of digeſtion in ſome of thoſe animals, loweſt in the ſcale of beings, as the frog, newt, ſnake, viper, *&c* Into the ſtomachs of frogs he introduced part of the ſmall inteſtine of a ſheep, the firſt animal ſubſtance which came in his way, incloſed in tubes. In the ſpace of a day, a conſiderable ſolution had taken place ; but in the more internal parts, it ſtill retained the characteriſtic marks of fleſh. In a longer time it was completely diſſolved, and that without any muſcular action. In ſome inſtances, however, a complete ſolution had not taken place, even at the end of the fifth day. This ſlowneſs, Abbé Spallanzani thinks, muſt proceed either from the ſmall quantity or inefficacy of the fluid, or perhaps from both cauſes. It would, however, appear, that the gaſtric fluid of the frog is capable of concocting even bones. At leaſt, of this our author had ſtrong evidence, from opening a frog which had ſwallowed a mouſe in its entire ſtate.

It was found by experiment, that the gaſtric fluid of the water-newt is much more ſpeedy in producing its effects than that of frogs. But in the ſtomach of this animal he found that there was very commonly two ſpecies of worms; and theſe, too, in conſiderable quantities. They were, in general, of a white colour, of the thickneſs of a thread, and ſome of them about two-thirds of an inch in length. In one, both extremities terminate in a point. The other has one end pointed, and one obtuſe, marked with a dark ſpot. Theſe are not looſe in the cavity of the ſtomach, but are always found with one extremity inſerted to ſome depth in the internal coat of that organ. The preſence of theſe worms is, our author thinks, an incontrovertible proof, that no ſenſible degree of force is exerted by the ſtomach of theſe animals: For it is impoſſible to conceive, that the ſides of the ſtomach can rub againſt each other, without injuring the delicate ſtructure of theſe worms; ſince, in conſequence of very gentle compreſſion of the ſtomach, between the finger and thumb, ſome rupture or diſcontinuation in the parts of the worms is always to be found. He accounts for their not being digeſted in the ſtomach, from the inability of the gaſtric fluid to decompoſe theſe minute beings, however powerful its energy may be upon others of a ſtructure leſs delicate.

In land-ſnakes he found the gaſtric fluid capable of digeſting the moſt tough parts of a wall-lizard, when confined in tubes forced into its ſtomach; and that this proceſs was more quickly effected in proportion, as the meat was leſs tough, and the gaſtric fluid had quicker acceſs to it. The ſtomach of the water-ſnake he found capable of diſſolving not only the ſofter parts of frogs, but their bones alſo. When he examined their gaſtric fluid, as obtained by ſponges, he found that its colour approached to that of ſoot; it had the fluidity of water, and evaporated very ſlowly; it had a ſalt and bitter taſte, and was not inflammable. Hence it appears, that it had a very ſtrong reſemblance to the gaſtric fluid of other animals. From the introduction of the tubes into the ſtomachs of vipers, the ſame effects were obſerved as in land and water ſnakes. From experiments, he found that no digeſtion took place in the œſophagus of theſe animals, and that their digeſtion was quickeſt in warm weather. But even in theſe caſes, where fleſh was for the longeſt time retained in their ſtomachs, it was not ſubjected to putrefaction.

After his obſervations on ſerpents, our author next proceeds to treat of digeſtion in fiſhes; and, taking thoſe fiſhes firſt, which have the neareſt reſemblance to ſerpents, he begins with the eel. He found, that portions of fiſh, incloſed in tubes, were diſſolved in the ſtomach of this animal in no long time. The ſame was alſo found to happen in the ſtomachs of animals more juſtly entitled to the appellation of fiſhes, as carp, barbles, and pikes. But from the diſpoſition which they have to vomiting, moſt of the tubes were rejected in a very ſhort time. In his examination of fiſhes, however, he had frequently occaſion to open thoſe which had not long before ſwallowed other fiſhes in an entire ſtate; and, from the phænomena there exhibited, it evidently appeared, that the bottom of the ſtomach digeſts more quickly than the parts ſituated above; that the œſophagus as well as the ſtomach is in ſome degree capable of concoction; and laſtly, that digeſtion in the œſophagus is ſlower both at its beginning and in its progreſs. In all the experiments made on fiſhes, there was nothing which could be conſidered as giving the ſmalleſt evidence of a triturating power.

From what have generally been ſtiled cold animals, Abbé Spallanzani proceeds to ſome experiments on the ſtomachs of warm animals, ſuch as ſheep, oxen, and horſes. In treating of this ſubject, he began by repeating ſome experiments formerly publiſhed by Mr Reaumur; and when he incloſed hay and graſs in his tubes, he found, as had formerly been obſerved by that philoſopher, that their contents were not diſſolved; and that too, even although they were allowed to paſs through the whole tract of the alimentary canal. From this he was about to declare in favour of the neceſſity of trituration, when it occurred to him, that the matters here employed had not been ſubjected to rumination. He imitated this proceſs then, by previouſly maſticating the herbs, and thus thoroughly impregnating them with ſaliva. He gave to a ram ſix tubes filled with this matter, and ſix others filled with the ſame herbs, not previouſly maſticated. Of theſe ſome were diſcharged by vomiting, ſome by ſtool, and others were taken out of the alimentary canal of the animal, when killed, at the end of two days. He now found, that thoſe vegetables which had been previouſly maſticated, were diſſolved, while thoſe which had not been maſticated were paſſed unchanged. Experiments made on corn were followed by the ſame reſult. From theſe and ſimilar trials, our author concludes that the gaſtric fluid of the ſheep has no effect in digeſting plants, unleſs they have been previouſly maſticated; but that this fluid is abundantly capable of digeſting plants, provided they are previouſly reduced to pieces by maſtication. And he concludes, that a triturating power in the ſtomach does not at all contribute to digeſtion in the ſheep, as the tubes found in the inteſtines, and voided by the anus, had ſuſtained no injury, although they were ſo weak that preſſure between the fingers was ſufficient to flatten them.

From trying artificial digeſtion with the gaſtric fluid taken out of the body, and from comparing its influence with that of water, it appeared, that it did not act on plants as an aqueous fluid, but as a real ſolvent.

Our author's enquiries concerning the digeſtion of ruminating animals, were cloſed by ſome experiments on oxen and on horſes. In the ſtomachs of theſe animals alſo, maſticated herbs, incloſed in tubes, were diſſolved without any injury to the tubes themſelves.

This diſſertation is concluded with ſome reflections on the near reſemblance between the ruminating ſpecies of animals, and birds endowed with muſcular ſtomachs, with reſpect to the action of the gaſtric fluid. In both, he obſerves, the fluid requires an agent capable of triturating and breaking down the food, before it can diſſolve and digeſt it. From the mouths of granivorous birds, where it undergoes

goes no alteration, it paſſes immediately into the craw, where it is ſoftened and macerated. From this receptacle it deſcends into the ſtomach, the muſcular power of which performs the office of teeth, triturating and grinding it, by which it is rendered fit to be diſſolved by the gaſtric fluid, and converted into chyme. In ruminating animals, Nature, he obſerves, employs a ſimilar proceſs. The hay and graſs deſcend immediately into the firſt and ſecond ſtomachs in nearly the ſame ſtate as when they were browſed. Here they are ſoftened by the exuberance of the gaſtric juices, as ſeeds in the craw of birds. But as the ſtomachs of ruminating quadrupeds have no ſenſible triturating power, and as the aliment requires trituration, Nature has wiſely provided for this, by cauſing it to aſcend, in conſequence of a gentle ſtimulus to vomit, into the cavity of the mouth, where, by means of rumination, it receives the neceſſary prediſpoſition to be digeſted by the gaſtric fluid.

In the proſecution of his experiments on animals with membranous ſtomachs, Abbé Spallanzani proceeds, in his fourth diſſertation, to treat of digeſtion in the ſtomach of the owl, the falcon, the eagle, and other birds of prey.

Here he ſets out with an obſervation which he had an opportunity of making, from having given ſome ſparrows to his owls. Theſe were ſwallowed whole, and of courſe both the feathers and food of the ſparrows, not yet digeſted, were received into the ſtomachs of the owl. After the fleſh had been digeſted, the feathers were vomited, in general in the form of a hard ball; and along with the feathers, the grain which had been the food of the ſparrow, and which, although much ſoftened by maceration, yet continued whole. From this it appears, that the ſtomach of this bird is really membranous, and without any power of trituration; but that, at the ſame time, it is capable of diſſolving bones. This was ſtill farther demonſtrated, by putting the thigh of a pigeon into one of the tubes, and giving it to an owl. In due time, both the fleſh and bone were completely diſſolved.

From introducing ſponges into the ſtomachs of owls, he found, that Nature has provided in theſe animals, a very large ſupply of gaſtric liquor. This liquor was of the fluidity of water, but had a reddiſh colour like the yolk of an egg. But this colour depended on corpuſcules floating in it, which ſoon ſubſided, leaving a tranſparent fluid. From repeated examination, however, he diſcovered that it has always this colour in the ſtomach of the animal. From trials out of the body, he found, that it ſoon diſſolved part of the inteſtines of a calf, which he employed for trying its power. But, notwithſtanding this power of the gaſtric juice, in its natural ſtate, in a diſeaſed owl, which died a few days after the experiment, he found, that animal ſubſtances contained in tubes, introduced into the ſtomach, were in no degree diſſolved.

After theſe experiments, on nocturnal birds of prey, Abbé Spallanzani next turned his attention to ſome diurnal ones. Fragments of the thigh-bone of an ox, incloſed in one of the tubes, and given to a large falcon, were gradually diſſolved; but while the ſurface ſeemed to be removed, ſtratum after ſtratum, it did not appear that the gaſtric fluid of the falcon had any influence in ſoftening theſe bones. He next tried whether the ſtomach of this animal was capable of diſſolving the enamel of the teeth. With this view, he incloſed two inciſores from the lower jaw of a ſheep, in a tube, which was retained in the ſtomach of the falcon upwards of three days before it was diſcharged by vomiting. Upon examining the teeth, he found that wherever the enamel did not extend, they were corroded and waſted, but the other parts were uninjured, and as brilliant as at firſt. After four days longer continuance in the ſtomach, upon being introduced a ſecond time, the fang was nearly diſſolved, though the enamel was perfectly ſound. Hence he concludes, that the gaſtric juice of the falcon, although a very powerful ſolvent, even for the hardeſt bones, is incapable of acting on the enamel of the teeth; a circumſtance not ſurpriſing, ſince that matter differs from every other oſſeous ſubſtance.

Small pieces of ſheeps and oxens horns, and likewiſe of the cartilaginous covering of the gizzard of gallinaceous fowls, were not diſſolved in the ſtomach of the falcon. But a portion of the tendo Achillis of an ox, one of the moſt tenacious tendons that is to be found in animal bodies, even when ſo dry that a keen knife would hardly cut it, was completely diſſolved, even when incloſed in the tubes. In like manner, calves ſkin and oxens hide were both readily diſſolved; but this does not happen if they have been tanned, and are in the ſtate commonly uſed for making ſhoes. Liſts, however, of ſheep-ſkin, when dreſſed, and even when dyed yellow, were completely digeſted in a few hours; while, at the ſame time, from repeated trials, it was found, that a variety of vegetables, as crumbs of bread, chick-peaſe, apples, pears, or the like, ſuffered no alteration in the ſtomach of the falcon. By means of its gaſtric fluid obtained in ſponges, artificial digeſtion was effected out of the body, when the materials were kept in the common temperature of theſe animals.

After relating his experiments on the falcon, our author proceeds to give an account of experiments which he made on the eagle. With this animal, the reſult of the experiments was not very different from what has been related above. It was, however, found, that the ſtomach of the eagle could diſſolve bread, although incapable of acting upon grain in its entire ſtate. From different trials, it appeared, that, with this animal, fleſh is only macerated in its craw, and that the whole proceſs of digeſtion begins and ends in the ſtomach. It was found, that the ſolution of fleſh was effected entirely by the gaſtric liquor acting upon the ſurface, and diſſolving one ſtratum after another, and in no degree by trituration. It was incapable of acting on the enamel of the teeth; but the hardeſt bone, as, for example, a ſphere formed from the thigh-bone of an ox, was diſſolved ſooner in the ſtomach of the eagle than in any other bird of prey. This, however, Abbé Spallanzani is diſpoſed to attribute, not to any ſuperior ſolvent power in the gaſtric fluid of the eagle, but to its greater quantity; for with this animal, he obſerves, it is ſo abundant, that a conſiderable quantity of it is ſpontaneouſly vomited every day. Its colour, in place of yellow, was cineritious; but in ſmell, and other particulars, it very much reſembled the gaſtric liquor of other birds of prey.

Out of the body, the gaſtric fluid of the eagle was found to diſſolve, not only ſoft animal and vegetable matters, but even bone. It completely diſſolved alſo the inflammatory cruſt taken from the blood of a pleuritic patient. And when it was expoſed to cold, he found, that it did not freeze ſo ſoon as either common water, or water impregnated with ſuch a quantity of ſalt as to give it a ſtronger taſte than the gaſtric fluid had. When the fluids thus frozen, were again expoſed to heat, the firſt that thawed was the gaſtric fluid, the next the brine, and the laſt the water. As from this experiment it was evident that the power which the gaſtric juice has of reſiſting cold more than water, cannot be entirely attributed to its ſaline impregnation, it muſt neceſſarily, he thinks, contain ſome other principle retarding congelation. Although he could not obtain the gaſtric fluid of other animals in ſufficient quantity to enable him to repeat this experiment with them, yet the cloſe analogy which they have to the gaſtric liquor of the eagle, renders it, he thinks, probable, that they contain a ſimilar principle.

In the fifth diſſertation, our author concludes his obſervations on digeſtion in animals with membranous ſtomachs, by relating the experiments he made on the cat, the dog, and the human ſpecies.

He found great difficulty in getting tubes introduced into the ſtomachs of cats; but having at laſt ſucceeded, both with tubes filled with fleſh and bread, irrefragable proof was afforded, that the gaſtric fluid is, in this animal, the efficient cauſe of digeſtion, independently of any triturating power.

It has been a common opinion, particularly ſince the time of Dr Boerhaave, that dogs are incapable of digeſting the inteſtines of different animals, or ligaments; and from the ſtate in which theſe are ſometimes diſcharged, there is ſome reaſon for adopting this idea. But our author diſcovered that this aroſe only from their paſſing quickly through the alimentary canal, and not having continued in the ſtomach for a ſufficient length of time; for having incloſed theſe ſubſtances in large tubes, which could not paſs through the pylorus, he found, upon killing the dog who had taken them, at the end of four days, that they were completely diſſolved.

The experiments of Drs Boerhaave, Pozzi, and others, tend to ſhew, that bones are not ſubjected to ſolution in paſſing through the alimentary canal of a dog. But from incloſing bones in tubes, which were allowed to remain in the ſtomach of a dog for ſeven days before he was killed, our author found them almoſt entirely diſſolved, and what remained was ſo far altered, that it could eaſily be cut by a knife.

Upon frequently repeating this experiment, however, it was found, that in one inſtance, a ſmall diminution of the bones only was produced in eight days, though the animal ſeemed to be in perfect health the whole time. This, however, only ſhows, that the digeſtive powers are not always equal, while, from the other experiments, it was incønteſtably proved, that the ſtomach of the dog is capable of digeſting bone as well as fleſh, though the latter, on account of its ſoftneſs, is more ſpeedily diſſolved. Among other particulars, Abbé Spallanzani found, that, in one dog, the enamel of two of the dentes inciſores was corroded in ſeveral places, while, at the ſame time, a linen bag in which they were incloſed had ſuſtained no injury. Thus, it appeared that the gaſtric fluid of the dog had more effect than thoſe of the falcon or eagle, which were found to be very powerful ſolvents of bone.

Our author concludes his reſearches on animals with membranous ſtomachs, by experiments on the human ſpecies; and here he made himſelf the ſubject of trial. From ſwallowing both vegetable and animal matters, ſome with, others without maſtication, incloſed in bags of linen, and in wooden tubes, he had undeniable proof, that digeſtion is produced in the ſtomach by the gaſtric fluid, independently of trituration. From theſe experiments, however, it appeared, that maſtication very much promotes ſolution in the ſtomach. In the ſame manner, trials were made on membranes, cartilage, and bone, incloſed in tubes. The two former, and bones of a ſoft texture, were completely diſſolved, although they required a conſiderable time; and before a complete ſolution could be effected, they were made to paſs two or three times through the alimentary canal. But bones of a hard texture, underwent no perceptible diminution of weight, even after being lodged in the alimentary canal for upwards of eighty hours. Although, however, Abbé Spallanzani conſiders the gaſtric juice as the efficient cauſe of digeſtion in man, yet he allows, that the ſolution of the vegetable and animal ſubſtances, incloſed in tubes, is perfected in the inteſtines; and he imagines, that a fluid ſeparated from theſe organs poſſeſs alſo a ſolvent power.

After making theſe experiments in the body, our author was next deſirous of obtaining the gaſtric fluid in a pure ſtate, that he might make ſome experiments with it out of the body. For this purpoſe, after long faſting, he excited vomiting, by tickling the fauces, without drinking any warm water. The fluid thus obtained, when firſt diſcharged, was frothy and ſomewhat glutinous. After being at reſt a few hours, and depoſiting a ſmall ſediment, it was as limpid as water. To the taſte it was falt, but not bitter, and it ſhewed no token of inflammability. It evaporated in the open air, and when put on hot coals, it emitted a thick ſmoke; and a ſmall portion of it, when kept above a month, in the hotteſt ſeaſon of the year, ſuffered no change, either of colour or taſte, and did not acquire any bad ſmell. From trying another portion of it, with maſticated boiled beef, in the way of artificial digeſtion, he found that it acted both as a ſolvent and antiſeptic.

The volume before us is concluded by a ſixth diſſertation, the ſubject of which is to enquire, whether the food ferments in the ſtomach. But after having extended our remarks on the former diſſertations to ſo great a length, we ſhall here content ourſelves with preſenting to our readers the reſult of his obſervations on this ſubject. He remarks, that of the three ſpecies of fermentation, eſtabliſhed by modern chemiſts, the ſweet, the acetous, and the putrid, none takes place in digeſtion; that though this function is ſometimes accompanied with an acid, yet this principle diſappears entirely towards the concluſion of it; that putrefaction never in health attends digeſtion; but that the gaſtric fluid is a real antiſeptic.

After the analyſis which we have now given of this volume, it is, we preſume, unneceſſary to add, that the intelligent reader cannot fail to derive, from a careful peruſal of the work itſelf, much information, no leſs curious than uſeful.

II.

Eſſais ſur l'Hygrometrie, par Horaçe Benedict de Sauſſure, *Profeſſeur de Philoſophie à Geneve.* 8vo. Neuchatel.

THE improvement of the hygrometer is a ſubject which has long engaged the attention of philoſophers. But hitherto theſe attempts have been in a great meaſure fruitleſs.

Mr de Sauſſure, therefore, conſidered it as a ſubject well meriting his attention; and from his eſſay reſpecting it, he has obtained great reputation. As we have not yet been able to obtain the original work, we ſhall here preſent our readers with an account of it, extracted from the analyſis given of it in the "Commentarii de rebus in ſcientia naturali et medicina geſtis Lipſiæ, Vol. XXVI. Part 1."

The author having been, by a bad ſtate of health, confined to his chamber for near the ſpace of a year, employed this time in experiments on the hygrometer; and particularly, in attempts to conſtruct an hygrometer of human hair. This ſubject had formerly engaged his attention; but he had deſiſted from his attempts at that time, upon finding that hair, although at firſt very well ſuited for this purpoſe, was ſubjected to conſiderable changes in the courſe of a few months. But upon again renewing his enquiries, he found ſuch a remedy for this inconſtancy, as to be able to conſtruct hygrometers admitting of an accurate compariſon with each other, or being what philoſophers call *comparable inſtruments.* He has turned his attention alſo to what may be called the *vera hygrometria*, or the art of meaſuring the real and abſolute quantity of moiſture ſuſpended in the air; and he has likewiſe laboured the ſubject of evaporation. Of all theſe he treats in the preſent eſſay, which is divided into four ſpecimens, as he ſtyles them.

He begins the firſt of theſe ſpecimens, which is divided into ſix chapters, by the deſcription of a hygrometer conſtructed of human hair.

Such is the nature of hair, that when moiſtened, it becomes longer; when dried, it becomes ſhorter. The difference in the extremes between the greateſt length obtained by moiſture, and the ſhorteſt obtained by drying, in a hair weighing three or four grains, and properly prepared by lixivium, is twenty-four or twenty-five thouſand parts of its whole length. In conſequence of this property, Mr Sauſſure made it the baſis of hygrometers on two different conſtructions, the one of which he ſtyles the *large*, the other the *portable hygrometer*. The larger hygrometer, or that made with a moveable cylinder, is conſtructed of a hair, fixed at one extremity, and having the other rolled round a cylinder, which is moveable upon its axis, a weight of three or four grains being ſuſpended to the looſe extremity of the hair below the cylinder. The cylinder, which muſt neceſſarily be rolled round by every change in the length of the hair, carries along with it an index, which, by its motion on a circle properly graduated, points out the degree of change which has taken place in the length of the hair.

But the accuracy of this hygrometer is always to be ſuſpected, unleſs it remains immoveable in its ſtation. For if it be removed from one place to another, the light hair eaſily ſlips, and moves the index, without any change in the quantity of moiſture. Hence he was led to invent a portable hygrometer, which is conſtructed of an index fixed on a very fine axis. The oppoſite part of the index beyond the axis, terminates in the ſector of a circle, the centre of which is the axis. The arch of this ſector at its loweſt part, has the one extremity of the hair fixed to it. The other extremity of the hair is fixed to the upper part of the inſtrument, and the hair is kept upon the ſtretch, by a proper weight ſuſpended at its inferior extremity. When the hair, therefore, is extended by moiſture, this weight will bring down the ſector of the circle, and raiſe in proportion the index on the oppoſite ſide of the axis. A contrary effect will be produced from dryneſs. Thus, in this inſtrument, the aſcent of the index marks the degree of humidity, and its deſcent, the degree of dryneſs.

In the ſecond chapter, the author treats of the ſelection and preparation of hairs for making hygrometers. Theſe, he obſerves, ſhould be taken from a ſound and living body, pure, ſoft, clean, pellucid, and not adhering together or contorted. As, however, to every hair, even with all the properties mentioned, a quantity of fat adheres, he adviſes that it be boiled for the ſpace of twenty-five or thirty minutes, in a lixivium of water and ſal ſoda, each ounce of the water containing about ſix grains of the ſalt.

For aſcertaining the point of greateſt humidity, he puts his hygrometer in a glaſs bell, placed above water, and having its inſide always wet. Thus, the air included in the bell, being every where ſurrounded with water, receives and diſſolves water till it be completely ſaturated. The hygrometer, therefore, placed in this air, points out the greateſt degree of humidity that air can obtain. An hair newly boiled, uſes gradually to be extended for two hours before it arrives at this point.

In dry air, an hair is extended by heat, for heat expands every body. In moiſt air it is contracted by heat, which dries both the air and the hair alſo. Mr Sauſſure, therefore, uſed the following method for finding the point of greateſt dryneſs. He dried the air, included in the bell, by introducing into it a plate almoſt red hot, with nitre and ſalt of tartar upon it, and keeping it there till the hair was no longer contracted by the heat, but rather expanded. His hygrometers were divided into an hundred degrees between the point of greateſt dryneſs and the point of greateſt moiſture. He placed the cypher at the point of greateſt dryneſs, ſuppoſing that at this point there is a total want of humidity; and the number of degrees in the hygrometer is thus ſuppoſed to increaſe with the quantity of the moiſture.

In the ſecond ſpecimen, our author treats of the principles and theory of hygrometers. Three methods have chiefly been propoſed by the learned, for aſcertaining the humidity preſent in the air. The firſt of theſe is, that bodies imbibing moiſture, as ſalt, ſponge, hair, or the like, ſhould be expoſed to the air, which is ready to communicate its moiſture. This moiſture paſſing into the bodies expoſed to it, changes their weight, figure, or length, in ſuch a manner, that, by the degree of change, the quantity of moiſture may be determined. Another method of determining the humidity of the air is, that water ſhould be expoſed to it for ſome length of time, by which the air abſorbs and receives part of the water; and by the diminution of the weight of the water which thus enſues, a determination may be formed what quantity of water the air could ſtill take up; and, therefore, how far it was ſtill diſtant from the point of greateſt humidity. This method of inveſtigating the humidity, however, he repreſents as particularly uncertain and fallacious. The third method is by condenſing the humidity contained in the air, by the aid of cold, and converting it into water. The moiſture of the air may then be determined by the quantity of water which a certain degree of cold induces on the ſurface of veſſels of a determined magnitude, or from the degree of cold which is neceſſary for producing the precipitation of the humidity. But this method is both difficult, and applicable only to a few caſes.

Our author proceeds, therefore, in the ſecond chapter, to the examination of an hygrometer conſtructed of human hair. He mentions, as the eſſentials of a good hygrometer, that it ſhould be remarkable both for the greatneſs and quickneſs of its changes; that theſe changes ſhould correſpond with thoſe of other hygrometers, and with the changes to which it is itſelf ſubjected at other times; that it ſhould be ſubjected to changes from no other cauſe, excepting moiſture; and that the variations which it exhibits ſhould be proportional to the changes of moiſture in the air. In theſe different particulars, he alleges, hygrometers formed of hairs excel all others.

To determine whether a hygrometer formed of hair was affected by any other vapour than moiſture, Mr Sauſſure incloſed ſuch an hygrometer, together with a thermometer, in a glaſs bell, placed upon pure quickſilver: And with theſe inſtruments, he put in the different matters on which he wiſhed to make trials. He found that thoſe ſubſtances which contain a proportion of humidity, as camphor, oil of turpentine, æther, ſpirit of wine, and the like, rendered the hair ſomewhat longer; while again fatty oils, wax, volatile alkali in its concreted ſtate, and the like, did not produce the ſlighteſt change on the length of the hair. Hence he concludes, that the hairs are not affected unleſs by humid vapours.

Philoſophers have differed much with regard to the influence of rarefied and condenſed air on the hygrometer. Our author, therefore, diligently enquired into this ſubject. He found, by very accurate experiments, that air rarefied by the air-pump became drier, although, during the time of exhauſtion, vapours eſcape from the inner part of the air-pump, which render the included air a little moiſter. When theſe vapours, however, are excluded or removed by the aid of a proper valve in rarefied air, the hygrometer always ſhews a greater degree of dryneſs than in common atmoſpheric or condenſed air. This he aſcribes to the vapour in any given portion of air being rendered thinner when the air is rarefied, and thus the vapour is incapable of acting with the ſame degree of force on bodies expoſed to it. But when air is completely dried by the aid of ſalts, its rarefaction and condenſation do not produce any change whatever on the hygrometer.

Mr Sauſſure obſerved, that when his hygrometer in the open air was expoſed to a ſlight breeze of wind, it ſhewed a greater degree of dryneſs; and that upon the wind ceaſing, it immediately returned to its former ſtate. From this he ſuſpected, that when the air was in motion it diſſolved, and as it were received into itſelf a greater degree of moiſture than when at reſt. To determine this, he put in motion, by means of a proper machine, the air included within a glaſs bell. In this caſe, he found that the hygrometer was indeed affected a little; but ſo little, that he attributed it entirely to the heat produced by the friction of the inſtrument. Hence he infers, that the motion of the air has no effect on the hygrometer, and that the dryneſs perceived to ariſe from a ſlight blaſt of wind, was to be aſcribed to the wind bringing dry air from the higher regions of the atmoſpere.

It ſeemed natural to ſuppoſe, that electricity, the power of which in augmenting evaporation is well known, would have ſome effect on the hygrometer. But by experiment this was not found to be the caſe.

In the third ſpecimen, our author preſents his readers with a theory of evaporation. All vapours, according to our author, are effluvia ariſing from different bodies, paſſing into the air, and ſuſpended there, till, by ſome cauſe, they are ſeparated from the air, and, joining together, acquire a more denſe form. There is no body, whether natural or artificial, which may not be reſolved into vapour; and hence acid alkaline, ſpirituous vapours, and the like. But here, it is the intention of the author, to treat only of watery vapours, which he divides into four claſſes. Theſe he ſtyles pure elaſtic, diſſolved, veſicular, and concrete vapours.

In the firſt chapter of this ſpecimen, he treats of elaſtic vapours, and their diſſolution in the air. Such is the nature of water, that, changed by the power of heat, it paſſes into vapour, which, cooled again, returns into drops of water. But under the influence of the heat of boiling water, it remains tranſparent and elaſtic, and exhibits all the mechanical phænomena which are peculiar to air. It is, however, the common opinion of philoſophers, that all the elaſticity of thoſe fluids, which have the appearance of air, ariſes from elementary fire mixed with them. But this elementary fire ſeems to be more immediately conjoined with other aeriform fluids, than with elaſtic watery vapour: For this vapour, by the mere action of cold, is condenſed and reduced to its priſtine form of water; while other aeriform fluids do not loſe their elaſticity merely by the action of cold.

Suppoſing elementary fire, united with the particles of the body diſſolved, to conſtitute elaſtic vapour, if the force of the fire be ſo great as to overcome the preſſure of the atmoſphere, and other external obſtacles, it conſtitutes pure vapour; but when the power of the fire is weaker, ſo that it cannot move the adjacent air from its place, then this vapour is mixed with and diſſolved in the air. And this mixture and diſſolution, is, he thinks, the true and only cauſe of evaporation. For, in his opinion, air does not diſſolve water itſelf; this chemical ſolution he ſuppoſes to take place only, when water, by the aid of fire, paſſes into elaſtic vapour. The motion of air promotes and accelerates this ſolution of vapour. Hence the remarkable power of wind in drying wet bodies. For wind conſtantly renews the adjacent air, and brings new air not ſaturated with moiſture. Heat promotes evaporation from theſe circumſtances; becauſe it ſupplies a greater quantity of elementary fire; becauſe it increaſes the ſolvent power of the air; and becauſe it aſſiſts the motion of the air. But the greater the heat is, the leſs is the elaſtic vapour mixed with the air; for by the heat of boiling water, the vapour is kept in its pure ſtate.

In the ſucceeding chapter, he treats of veſicular and concrete vapour. Vapour which is not diſſolved by the air, is depoſited on the ſurfaces of cold bodies, and paſſes into the form of dew or cryſtals, the quantity of which depends on the affinity of theſe bodies with water, and on their ſtate of electricity. But when a cold body is not preſent, theſe vapours are joined among themſelves, and paſs into the form of ſmall drops or of veſicles. Thoſe ſmall drops which conſtitute the firſt elements of rain or ſnow, are diſtinguiſhed by our author by the name of concrete vapour. From the aſcent of clouds, Mr Sauſſure thinks, it is abundantly evident, that veſicular vapours are lighter than air. For that clouds conſiſt of mere veſicles and not of drops, is, he thinks, very evident, from the refraction of the rays of light being different in clouds from what it is in rain itſelf. Our author thinks it probable, that theſe veſicles are each ſurrounded by an atmoſphere of its own, and that this atmoſphere conſiſts of the ſame matter with that which fills the interior part of the veſicle, whether this be fire, electrical matter, or ſome new and hitherto unknown elaſtic aeriform fluid. He allows, that it is a difficult matter to explain the origin of theſe veſicles; but their water would appear to remain in a fluid ſtate, and to reſiſt congelation more than water in other ſituations, ſince clouds are to be ſeen in very cold weather. When theſe veſicles are broken down, their water paſſes into hoarfroſt, rain, or ſnow.

Theſe veſicles do not appear in any other air than that which is ſaturated with moiſture; and if the ſolvent power of the air be increaſed, the veſicles vaniſh. On the other hand, when the atmoſphere is completely ſerene, clouds ſometimes ſuddenly appear; from which it may be inferred, that in air ſaturated with water, little is requiſite for its aſſuming the veſicular form.

He next treats of evaporation in rare and in denſe air. In rarefied air, he obſerves, vapours ariſe moſt eaſily, on account of the ſlighter preſſure; yet, in this air, a leſs quantity of water can be diſſolved. Hence, in rarefied air, the evaporation is leſs, if the water be not very warm, and a perpetual diſtillation preſerved by oppoſite cold ſurfaces.

The ſubject of enquiry which next engages his attention is, whether the paſſage of fire from terreſtrial bodies into the air, is to be ranked among the cauſes of evaporation. Many eminent philoſophers have been of opinion, that fire, eſcaping from bodies, mechanically carries moiſture along with it, and that thus evaporation is produced. In this manner they have attempted to account for the vapours obvious in the evening, ſuppoſing that at ſunſet, the earth cooling, gives out its fire, carrying a great quantity of moiſture along with it. This appearance, however, our author aſcribes to the cooling of the air, which cannot longer retain in a ſtate of ſolution the former quantity of fluidity. By the warm winds which happen about the end of winter, the earth is heated. By theſe winds, however, it is not moiſtened but dried; from whence he thinks it is evident, that by fire paſſing into the earth, moiſture is not inſinuated but expelled.

After ſome obſervations on the evaporation of ice, which our author contends is diminiſhed in proportion to the increaſe of cold, and on the evaporation of impure water from a mixture with other matters, in which he takes notice the influence of ſalts, ſome as diminiſhing, others as augmenting evaporation; he concludes this ſpecimen with a general review of his theory of evaporation. Evaporation, properly ſo called, he conſiders as ariſing from the intimate mixture of elementary fire and water. Theſe elements, intimately connected with each other, paſs into an elaſtic matter, more rare than atmoſpheric air, which philoſophers have diſtinguiſhed by the name of vapour. This vapour, if it ariſes in vacuo, or if, by its force and heat, it be able to overcome the preſſure of the atmoſphere, and to remove from its place, the air reſiſting it has the name of pure elaſtic vapour. But when it has not ſuch power as to be able to overcome the whole preſſure of the air, it penetrates

netrates into the air, is mixed with it, and diſſolved in it, and is then ſtyled elaſtic vapour diſſolved in the air. Air, again, ſaturated with ſuch vapours, often depoſits a ſuperfluous and ſuperabundant part under a veſicular form. Theſe veſicles being filled and ſurrounded with a very rare and light fluid, float in the air, and, on account of their very ſmall ſpecific gravity, ſometimes aſcend to a very great height. And laſtly, elaſtic vapour, and even theſe veſicles themſelves, are often ſo condenſed, that they aſſume the form of water drops, differing from drops of rain only in their ſize. But as they often float in the air for a conſiderable time before they fall to the ground, he thinks, that this water may not improperly be ranked among the vapours, and diſtinguiſhed by the title of concrete vapour. Under theſe four kinds of vapour are comprehended all the effluvia, which by aid of evaporation paſs from terreſtrial bodies into the air.

In the fourth ſpecimen, Mr Sauſſure enters into an enquiry into the cauſes of the variations of the barometer. After objecting to the opinions of Mr de Luc and others, he does not pretend to offer any certain cauſe, but merely to propoſe ſome general probable conjectures. He obſerves, that an hypotheſis which can give a probable explanation of the varieties in the barometer, muſt neceſſarily ſhew why theſe variations are very inconſiderable, or rather nothing at all in the torrid zone; while, again, towards the poles they are more and more increaſed and augmented. Here, therefore, regard is chiefly to be paid to theſe circumſtances which take place in the atmoſphere at the equator and towards the poles. Theſe, however, he thinks, may chiefly be referred to three particulars. *1ſt*, Under the equator, the temperature of the air is more ſteady, and not ſubjected to the ſame variations in heat as towards the poles. *2dly*, Under the equator, the winds are more conſtant and ſteady, and are ſubjected, as it were, to certain laws. *3dly*, The heat at the equator, in riſing from the lower to the higher parts of the atmoſphere, decreaſes more gradually than it does towards the poles. Hence, in the latter ſituation, there is greater viciſſitude and variety of heat in different ſtrata of the atmoſphere. From theſe circumſtances, Mr Sauſſure infers, that the chief cauſe of the variations in the barometer is connected with the temperature and with the winds.

When an hygrometer is employed for metereological obſervations, it muſt be expoſed to free air in ſuch a manner that it be neither much heated by the ſolar rays, nor very much in the ſhade. When obſervations are made in the fields, it may be conveniently enough fixed to a cane, the ſhade of which may keep off the rays of the ſun. In obſervations within doors, it is neceſſary that it ſhould be removed at ſome diſtance from any large maſſes of matter which are either hot or cold, moiſt or dry. And, in obſerving the index, a lens ſhould be employed, that the degrees of the ſcale may be more manifeſt. With regard to the period of the day for obſervation, he has, in general, obſerved, that the greateſt dryneſs takes place between three and four in the afternoon, and the greateſt moiſture about an hour before ſunriſe.

This treatiſe is concluded with an enumeration of ſome particulars which our author conſiders as ſtill wanting for improving hygrometrical obſervations. Though he conſiders the hygrometer conſtructed of human hair as the beſt, yet he adviſes that different kinds of hygrometers ſhould be combined and compared together; that more accurate enquiry ſhould be made into the quantity of vapour which each kind of air is capable of diſſolving, and of retaining in a diſſolved ſtate; and that experiments ſhould be made upon elaſtic vapour in vacuo. He thinks that an inveſtigation ſhould be made, by obſervations and experiments, into the nature of veſicles and clouds, that it may be determined what quantity of water fogs and clouds contain; and that a diaphanometer ſhould be invented, for determining the degree of pellucidity of the air. And, finally, he is of opinion, that the theory of evaporation ſhould be extended, that it may be determined, whether every body, when exhaling, furniſhes an elaſtic vapour, which can be diſſolved in the air, and aſſume a veſicular form. Theſe diſquiſitions, he thinks, would afford an entirely new field for hygrometry and meteorology.

III.

Phyſical and Chemical Eſſays, tranſlated from the original Latin of Sir Torbern Bergmann, *Knight of the Order of Waſa, Profeſſor of Chemiſtry at Upſal,* &c. *by* Edmund Cullen, *M. D. Fellow of the Royal College of Phyſicians at Dublin; with Notes and Illuſtrations by the Tranſlator.* Vol. I. 8vo. London.

IN the tranſlation before us, Dr Cullen has done great juſtice to the labours of that very ingenious and truly great chemiſt and philoſopher, Sir Torbern Bergmann. And as they are intended for the inſpection of the artiſt, as well as thoſe engaged in the profeſſion of medicine, he has added notes, to render ſome parts of them more eaſily underſtood by thoſe who are not complete adepts in the ſcience of chemiſtry, or who are unacquainted with all the new diſcoveries of that art.

This firſt volume of eſſays conſiſts of eleven diſſertations, on different very important practical ſubjects in chemiſtry. But as the limits of our work will not allow us to follow our author through all theſe in one volume, with ſuch a degree of minuteneſs as to be uſeful to the reader, we ſhall here preſent him with Mr Bergmann's obſervations on the firſt, and one of the moſt important ſubjects, the aerial acid.

In a preliminary diſcourſe on the inveſtigation of truth, the author explains the principal rules which he had laid down for himſelf. Theſe regard either the compoſition of bodies, or the explanation of phænomena.

With regard to the former, the rules are:

1*ſt*, In inveſtigating the principles of a body, he ſays, we muſt not judge of them from a ſlight agreement with other known bodies; but they muſt be ſeparated directly by analyſis, (*i. e.* decompounding the body, or reſolving it into its conſtituent parts); and that analyſis muſt be confirmed by ſyntheſis, (or recompounding the body, by uniting the parts formerly ſeparated).

2*dly*, Analyſis, he ſays, ſhould chiefly be conducted in the humid way; for though the dry way may ſometimes be uſeful, yet, in moſt caſes, fire tends rather to confound than to ſeparate different ſubſtances.

3dly, He adviſes to inſtitute ſuch experiments as are adapted to the diſcovery of truth.

4thly, Such experiments ſhould be made with all poſſible accuracy. And,

5thly, The experiments of others, particularly the more remarkable ones, ſhould be candidly reviewed.

The phænomena of a body, he obſerves, either conſidered by itſelf, or in reference to other bodies, have their foundation either in the body itſelf, or in external circumſtances: He concludes, the explanation of theſe muſt be ſought for; and to avoid falſehood, he propoſes the following rules:

1ſt, In the inveſtigation of cauſes, we muſt begin by phænomena ſufficiently varied, and well obſerved, and proceed in order from proximate cauſes to the more remote.

2dly, A cauſe any how indicated by phænomena, may, for a while, be aſſumed as true, and from it may be deduced the neceſſary conſequences, which being ſeparately examined by ſuitable experiments, either confirm or overturn the poſition.

3dly, The cauſe ſhould, if poſſible, be ſo compared with the effect, that the exact relation may be diſcovered even as to quantity.

4thly, Denominations given to things ſhould be as agreeable to truth as poſſible.

After theſe preliminary obſervations, he next proceeds to treat of what he calls the *aerial acid*, or what goes among Britiſh philoſophers by the name of *fixed air*.

The term *fixed air*, has, he obſerves, been uſed both in an extenſive and a limited ſenſe. In the former, it is taken to expreſs every elaſtic fluid ſet at liberty during the decompoſition of bodies, by whatever means it be extricated, or whatever be its nature.

Theſe fluids, however, he contends, are not all of the ſame kind, ſome being very ſuſceptible of inflammation, while others inſtantly extinguiſh fire. Yet they all agree in this, that though they reſemble common air in many properties, they appear to have exiſted as it were fixed in bodies, and deprived of their elaſticity, which they do not recover until the inſtant of extrication; hence they have been denominated *fixed*, in contradiſtinction to common air.

But the term *fixed air*, when taken in a more limited ſenſe, is employed to denominate that ſpecies of air which is found in alkaline earths, and ſalts, extricable by fire or acids, and which iſſues in great quantity from many vegetables under fermentation. This, he alleges, when properly depurated, poſſeſſes always the ſame qualities; and as it exhibits all the properties of an acid, and exiſts almoſt every where, and particularly in the atmoſphere, he adopts the term *aerial* or *atmoſpheric acid*.

This acid is the ſame with that ſpecies of air called *mephitic*; it is deſtructive to animal life, as has long been known with reſpect to that found in the cavern near Naples, called Grotto del Cane.

After this deſcription of the aerial acid or fixed air, he next proceeds to ſhow how it may be procured in its pure ſtate. He exhibits three different methods of doing this, which are not eſſentially different from thoſe deſcribed by Drs Black, Prieſtley, and others, *viz.*

1ſt, By expelling it from calcareous ſubſtances by means of a ſtronger acid, which is attended with effervescence.

2dly, By expelling it from magneſia alba, by a ſufficient degree of heat. Or,

3dly and *laſtly*, By catching it, as it ſpontaneouſly ariſes from fermenting ſubſtances.

For procuring pure aerial acid, he has contrived a proper apparatus.

The matters he uſes, are diſtilled water, pellucid calcareous ſpar, and vitriolic acid. He prefers this acid to any of the other acids, on account of its great fixity, particularly to the nitrous and marine acids, as theſe are ſo volatile that they would riſe in part along with the fixed air, and render it impure; and, for the ſame reaſon he cautions againſt the uſe of too much heat during the ſeparation. He likewiſe chuſes calcareous ſpar rather than chalk, the chalk always containing a quantity of marine acid, in conſequence of the volatility of which, the heat occaſioned by the ſeparation of the fixed air would make it riſe along with the aerial acid, and render it impure in the ſame manner as if marine acid had been uſed for extricating the fixed air.

For the ſecond method he uſes magneſia alba, which he puts into a green glaſs retort, placed in a crucible, ſurrounded with gypſum, and ſet in a portable furnace. The crucible is to be made red hot. The air bubbles which firſt appear, he allows to fly off, till the common air be diſſipated. The air which afterwards comes off, he collects in a proper veſſel adapted to the retort. Magneſia, he ſays, in this way, when ſcarcely ignited, emits its fixed air with eaſe.

To produce fixed air in the third method, he makes uſe of the following means of raiſing the fermentation.

In a bottle of 350 cubic inches capacity, he mixes twenty ounces of ſugar, and as much of good yeaſt, with 200 cubic inches of water; in ſix or ſeven hours, and in about fifteen degrees of heat (by the Swediſh thermometer, anſwering to fifty degrees of Fahrenheit) the common air is generally expelled. He then adapts the orifice of a crooked tube to the mouth of the bottle, having the other extremity of the tube fitted to a bottle immerſed in water, then air bubbles immediately ariſe into it. He found the fermentation go equally well on, if the tube be at firſt adapted to the neck of the bottle; the acceſs of air not being neceſſary, it is ſufficient that an exit be allowed to the elaſtic fluid.

It is neceſſary, he ſays, to take care in this as well as in the former methods, that the tube be not too long, as the difficulty of the proceſs increaſes in proportion to the quantity of air to be removed.

To illuſtrate this, our author gives us the following experiment:

"Prepare a ſtrong bottle nearly full of water; to which let chalk or cryſtallized alkali be added: Let it then be filled with any acid, and quickly and cloſely ſtopped. At firſt a few bubbles appear; but this appearance ſoon ceaſes, and the mixture remains clear, even for years, provided no exit be allowed to the elaſtic fluid, and ſuch a portion only be ſaturated that the water can take up its fixed air, but the ſmalleſt aperture immediately excites the moſt violent efferveſcence.

In all theſe different methods, he found the fixed air that was produced, exhibited exactly the ſame properties.

In order to aſcertain that fixed air is an acid, he enumerates the properties of acids in general, and then gives the different methods by which theſe properties are to be made evident to the ſenſes in fixed air.

The general properties of acids he takes to be the following:

1*ſt*, They readily unite with water.

2*dly*, They have an acid taſte.

3*dly*, They change the blue juices of vegetables to a red.

4*thly*, They have a ſtrong affinity with alkaline ſalts, and form with them compounds milder than either of the conſtituent parts.

5*thly*, They diſſolve ſeveral earths and metals.

6*thly*, They precipitate ſubſtances diſſolved in alkalis. And,

7*thly*, They often attract inflammable matter very ſtrongly.

Our author next proceeds to ſhew the different methods of combining the aerial acid with other matters, as water, fixed alkalis, volatile alkalis, terra ponderoſa, calcareous earth, magneſia, clay, iron, zinc, and manganeſe, with ſome other metals, and with inflammable matters. The proceſſes for all theſe combinations do not materially differ from thoſe deſcribed by other writers; but the reſult of them are marked with greater accuracy than perhaps has been done by any others.

With reſpect to the method of uniting the aerial acid with water, he uſes the following:

He takes the receiver, filled with pure fixed air, in any of the three methods deſcribed, in a place where the thermometer ſtands but a little above the freezing point, and he puts it into a veſſel of water, with its mouth depreſſed by means of weights, almoſt to the bottom of the veſſel, that the union may be accelerated by the preſſure; the water will riſe in the bottle by degrees, and fill it, the union depriving the fixed air of its elaſticity.

In a heat of 41° of Fahrenheit, water will abſorb more than its own bulk of fixed air; in 50°, a quantity ſcarce equal, and always leſs in proportion as the mercury ſtands higher.

The ſpecific gravity of aerated water to that of diſtilled water, he found to be nearly as 1,015 to 1,000, the thermometer ſtanding at 35° or 36°.

From the above particulars, he concludes, that the aerial acid, in common with other acids, unites readily with water; but, from its volatility, eaſily flies off, and that water may be deprived of it altogether, by half an hour's boiling. He finds alſo, that congelation ſeparates it readily and completely.

Water thus impregnated with aerial acid has an agreeable acidity, and is what he ſtyles the real ſpirit of the cold mineral waters. Hence he infers, that the cold mineral waters, ſuch as Seltzer, Spa, and Pyrmont, may be imitated by impregnating with the aerial acid, water which has a due proportion of ſuitable ſalts diſſolved in it.

He remarks that water ſo cold, as to be only a little above the freezing point, emits very little taſte or flavour, though ſaturated with fixed air; but when ſet for a few minutes in a temperature of about 60°, it gradually evolves its pungent and grateful acidity, owing to cold ſtrengthening the union of the two bodies, by which the ſapidity is blunted; whereas the heat looſening that union, the ſapidity is rendered more ſenſible. Alkaline ſalts, he found, deprived the water of its ſapidity, having a ſtronger attraction for fixed air than the water has: However a few drops of mineral acid reſtores immediately the pungency to the water, by ſetting looſe the aerial acid which again unites with the water.

Although, from the different experiments, he found fixed air to be a true acid, yet it turns out a very weak one. He found that it did not change any of the blue infuſions of vegetables, excepting that of turnſol, one part of water, ſaturated with aerial acid, making fifty parts of tincture of turnſol diſtinctly red; but from the volatility of this acid, this change of colour ſoon diſappears in a veſſel expoſed to the open air. But though it does not alter other blue infuſions of vegetables, he concludes this is no proof of its not being an acid, and only ſhews that it is weaker than other known acids; as we find the colour of indigo not altered even by concentrated vitriolic acid: And further, that its not altering the more unchangeable blue infuſions, is a proof that it is not a foreign acid, but one *ſui generis*, otherwiſe, when concentrated, it would act upon the more ſteady blue infuſions, and alter them, which he found it could not do.

Our author next proceeds to ſhow the inclination this acid has to join with alkaline ſalts; for which purpoſe he gives a proceſs for impregnating theſe ſalts with this acid.

He immerſes the mouth of the veſſel, filled with aerial acid, under the ſurface of water, nearly ſaturated with pure alkali of tartar. The lixivium gradually riſes in the veſſel. After the veſſel is full of the lixivium, by which it is known to have abſorbed all the aerial acid, he pours the ſaline ſolution into a proper veſſel, and ſets it in a dry place to cryſtallize. In a few weeks he found cryſtals adhering to the ſides and bottom of the veſſel, which neither ſuffer deliqueſcence in moiſt air, nor effloreſcence in dry, but always retain their tranſparency.

Theſe cryſtals, he found, required, in a moderate temperature, four times their quantity of water to diſſolve them. By calcination, they ſplit and loſe $\frac{52}{100}$ of their weight; but by a ſlow diſſolution in acids, they loſe $\frac{20}{100}$ only. Hence he found, that in an hundred parts of theſe ſalts, there are thirty-two of water, twenty of fixed air, and forty-eight of pure alkali. From this he found, that an hundred parts of pure alkali, require forty-two of fixed air to ſaturate them.

He thinks the efferveſcence which takes place upon the union of alkalis with acids, is owing to the expulſion of the aerial acid, by the ſuperior power of a ſtronger acid, by which it recovers its elaſticity that had been repreſſed;

it therefore riſes to the top by its ſpecific lightneſs, occaſioning a foam on the ſurface.

The taſte of the cryſtals of aerated vegetable alkali, he ſays, is alkaline, but mild without any acrimony; whereas pure alkali, or that deprived of fixed air, is exceedingly acrid: Hence they have got in ſuch a ſtate, the name of *cauſtic*. When they have been deprived of water, they deliqueſce in the air, and form the Ol. Tart. *per deliquium*. This generally becomes ſaturated with fixed air from the atmoſphere; in conſequence of which, it forms cryſtalline vegetations on the ſides of the veſſel.

Although the vegetable alkali attracts fixed air very powerfully, yet it is difficult to obtain a complete ſaturation; for in proportion as the number of cauſtic particles is diminiſhed, they are more widely diffuſed through the fluid maſs: Hence they more faintly attract, and more difficultly meet with the elaſtic fluid, a circumſtance which occurs in almoſt every ſaturation. During cryſtallization, ſome particles ſtill remaining cauſtic, ſully the water of the cryſtals, as is ſeen from the colour of the precipitate of corroſive ſublimate which this alkali produces.

Thus pure fixed alkali produces a ferruginous mercurial calx; but when fully ſaturated with fixed air, it produces a white one. The vegetable alkali perfectly cryſtallized, precipitates a whitiſh calx ſullied with yellow particles; whereas if theſe cryſtals be powdered, and expoſed to the air upon bibulous paper for ſome days, they looſe all their cauſticity, and precipitate ſublimate in form of a white powder. He alleges this white colour depends upon fixed air adhering to the mercurial calx.

Mercury diſſolved in the nitrous acid, produces different appearances according to the different methods of preparation. Thus, if the ſolution be effected in the cold, and with diluted acid, a conſiderable quantity of the phlogiſton remains adherent; whereas, if fire be employed, a great part of it flies off in red vapours. Now, the more the mercury is impregnated with phlogiſton, the paler is the precipitate made by fixed alkali; and, on the contrary, the leſs it is impregnated, the darker is the red colour it yields with cauſtic, and the more ſullied the white one with aerated alkali.

In experiments which require very great accuracy, our author makes uſe of alkali prepared

red from burnt cream of tartar, as this is generally free from the marine acid, which, as well as the vitriolic, is, in general, mixed with ſalts elixated from vegetable aſhes.

Our author next proceeds to treat of aerated mineral alkali. He ſays, fixed mineral alkali, when produced from marine plants, is commonly called *ſal ſodæ*, and is uſually loaded with ſo much fixed air, that without any further accumulation, it naturally forms cryſtals of eight ſides. He found an hundred parts of theſe cryſtals contain ſixteen of fixed air, ſixty-four of water, and twenty of pure alkali: From thence he concludes, that an hundred parts of pure alkali require eighty of aerial acid to ſaturate them, owing to this alkali being weaker than the vegetable, as he alleges that in all ſimple ſalts the milder they are, the more eaſily they are ſaturated. In proof of this, he found, that, for ſaturation,

100 parts of mineral alkali require		100 parts of pure vegetable alkali require	
Of vitriolic acid	177	Of vitriolic acid	78½
— nitrous	135½	— nitrous	64
— marine	125	— marine	51½
— aerial	80	— aerial	42

The ſame thing, he ſays, holds good with regard to acids.

For 100 parts of vitriolic acid require

Of pure vegetable alkali - -	127½
— mineral - - -	56½
— volatile - - -	42

He finds cryſtallized mineral alkali, in a dry air, loſes its water, together with its tranſparency, and falls down into a powder; but diſſolves in a moderate heat in about twice its weight of water.

He next proceeds to treat of the aerated volatile alkali. The cauſtic volatile alkali, he remarks, attracts water very vehemently; and hence is always, in a fluid ſtate, very penetrating, and exceedingly volatile; but may be reduced mild in the ſame way as the fixed vegetable alkali, and is then diſpoſed to cryſtallize.

The volatile alkali being weaker than the fixed, according to the general law of ſimple ſalts, he ſays, requires a greater proportion of fixed air to ſaturate it, 100 parts of this alkali, when pure, taking up nearly 105 of aerial acid; and hence it occaſions a more violent effervеſcence with other acids.

An hundred parts of concrete volatile alkali uſually contains twelve of water, forty-five of aerial acid, and forty-three of pure alkali.

He next proceeds to conſider aerated terra ponderoſa, which, he ſays, is the baſis of the ſpathum ponderoſum, or the marmor metallicum of Cronſtadt, and bears a ſtrong reſemblance to calcareous earth.

This earth diſſolves very readily in water, when almoſt free of the aerial acid. In this ſtate water will take up $\frac{1}{1550}$ of its weight; which ſolution he found change paper dipt in an infuſion of Brazil wood into a blue, and heighten the colour of paper a little tinged with turnſol; but when the menſtruum is ſuperabundant, it reddens the colour. He found calcareous ſpar loſe, on expoſure to a certain degree of heat, 0,45 of its weight, together with its power of efferveſcing with acids, and acquire at the ſame time acrimony and ſolubility in water, owing to the expulſion of the fixed air. If the aerial acid be introduced into a ſolution of calcareous earth or lime-water, he found the water grew turbid, and depoſit a powder which efferveſces with acids. Hence he contends, that calcareous earth may be conſidered as a neutral ſalt, an hundred parts of which contains thirty-four of aerial acid, eleven of water, and forty-five of pure calcareous earth.

The reaſon why lime impregnated with fixed air is inſoluble, ariſes, he thinks, from the ſame cauſe as the inſolubility of other neutral ſalts, ſuch as vitriolated tartar; this ſalt requiring ſixteen times its weight of water to diſſolve it; whereas, when the acid predominates, it becomes very ſoluble. The ſame thing takes place with aerated lime. If a few drops of lime-water be put into aerated water, a turbidneſs will enſue, from the lime ſaturating a part of the acid; but upon agitating the water, it ſoon becomes tranſparent again.

He remarks, that the heat which new burnt lime imparts to water, indicates no more than a looſe adherence of the matter of heat, which is repreſſed, and prevented from producing ſenſible heat, the ſame as when the properties of acids are repreſſed by their union with alkalis. At the ſame time, he remarks, that lime, which generates heat, is not entirely deprived of its fixed air, as the intimate nucleus is found to efferveſce with acids. But if the fixed air be entirely expelled, the lime becomes dead and inert;

and, although ſtill ſoluble, it neither ſplits in ſlacking, nor occaſions heat, owing, as he ſuppoſes, to the vehemence of the fire, to which the greater part of the matter of heat muſt yield, from its volatility. He remarks, that lime, as well as terra ponderoſa, magneſia, and alkaline ſalts when cauſtic, even after extinction with water, produce heat on mixture with acids, the reſiduum of the matter of heat, which the water was unable to expel, being, by the acid, ſet at liberty; but, when the ſalt is cryſtallized, no heat is produced.

He, however, alleges, that cauſticity depends on elective attraction, and is not derived from any adventitious matter. To this attraction he imagines all bodies are obedient, for he finds, the purer a body is, the more powerful is its effort towards an union with other ſubſtances; hence, acting with undiminiſhed force, it ſeizes with the greater violence theſe ſubſtances with which it has an affinity, and, if powerful enough, diminiſhes their former connection; hence it exerts a violent acrimony, which diminiſhes as it becomes ſaturated. When a full ſaturation takes place, all its acrimony vaniſhes, until the ſaturating ſubſtance is expelled; and he imagines the matter of heat rather repreſſes than quickens cauſticity.

Upon the whole, he concludes, that cauſticity depends on a powerful elective effort, together with the abſence of the ſubſtances, for which the cauſtic body poſſeſſes a very violent attraction.

He next proceeds to the conſideration of magneſia. That which is kept in the ſhops, although commonly called an earth, he affirms, is, nevertheleſs of a ſaline nature; for, in a moderate heat, diſtilled water is capable of diſſolving $\frac{1}{850}$ of its own weight; and, from analyſis, he found that the primary principles of magneſia are a peculiar kind of earth, ſaturated with aerial acid, along with a portion of water. He finds, that an hundred parts of magneſia contains twenty-five of aerial acid, thirty of water, and forty-five of pure magneſia, which appears to be a pure earth.

In one hundred pounds weight of magneſia, fully ſaturated with aerial acid, he found, that, thirty conſiſted entirely of that acid, which is more eaſily ſeparated than from lime, as it adheres more looſely to the magneſia than to the lime.

He remarks, that the common magneſia of the ſhops is prepared generally by precipitation from Epſom ſalts; but he ſays, if the alkali be well impregnated with aerial acid, the precipitation will be ſmall, particularly in a large quantity of water. The aerial acid, expelled from the alkali by the vitriolic, attacks the deſerted magneſia, and diſſolves it; but by a boiling heat, the ſuperabundant volatile menſtruum is diſſipated, the magneſia is brought to the point of ſaturation, and almoſt all precipitated.

Water well ſaturated with aerial acid, he ſays, diſſolves, in a moderate heat, about $\frac{1}{100}$ of its own weight of common magneſia, and double the quantity, if it be as fine as a precipitate.

Common magneſia, by proper calcination, loſes 0,55 of its weight; but vehement heat diſſipates even part of its earthy baſis; 0,25 is only loſt by ſlow effervеſcence with acids.

The ſolution of magneſia in aerated water turns tincture of turnſol red; but heightens that of paper ſlightly tinged by it; makes paper blue that has been ſtained with fernambuccum, but ſcarcely obſcures the yellow colour of turmeric. On the addition of an acid, innumerable bubbles appear. Cauſtic fixed alkali precipitates an earth which does not efferveſce with acids, provided the alkali be uſed in ſufficient quantity to abſorb all the fixed air; but if in a ſmaller proportion, an efferveſcence will take place. An alkali fully ſaturated with fixed air has no effect on the ſolution, unleſs the water be inſufficient to hold both diſſolved, in which caſe the magneſia ſeparates. Pure or cauſtic volatile alkali alſo throws down a precipitate, but one which always efferveſces, as this can only attract the ſuperfluous quantity of fixed air. With a ſolution of corroſive ſublimate, it grows milky, and gradually depoſits a ſmall quantity of white powder; afterwards, thin blackiſh cryſtals concrete, compoſed of mercurius dulcis, and a calx of mercury partially aerated. The ſmalleſt drop of a ſolution of mercury, made by heat in nitrous acid, throws down a copious yellowiſh brown precipitate; but the ſame ſolution, made without heat, yields a white powder, which, after ſome days, grows grayiſh, as, in this caſe, the metal has loſt but a little of its phlogiſton.

Fixed air, he obſerves, ſcarcely ever attacks pure clay, *i. e.* the earth of alum, dry and compact. Yet the precipitation of alum by aerated alkali, ſhews that it does not entirely refuſe a connection with it, when ſufficiently fine and comminuted; for the liquor, though well filtrated and clear, after ſtanding a few days in the open air, and in a degree of heat capable of expelling the aerial acid, grows turbid, and, by degrees, depoſits a ſmall quantity of earth, which had been diſſolved in the volatile menſtruum.

In the following table, numbers are added, proportioned to the weights of the different earths diſſolved by the ſame meaſure of marine acid.

Of pure clay	1, 0		
—— magneſia	2, 2	Aerated	5, 7
—— lime	3, 5	——	6, 0
—— terra ponderoſa	8, 7	——	13, 5

100 parts of aerial acid will ſaturate,
150 of pure magneſia,
162 —— lime,
926 —— terra ponderoſa.

Fixed air, in its elaſtic ſtate, does not, he obſerves, diſſolve, or even corrode, any of the metals, unleſs, perhaps, during calcination. But when united with water, it attacks ſome of them; *e. g.* Let iron filings be put into water, ſaturated with aerial acid; let the bottle, well ſtopped and inverted, be ſet in a cold place; after twenty-four hours, a portion of the iron will be diſſolved, ſo that the water will ſtrike a purple colour with galls, and exhibit other marks of containing iron. When the ſolution is expoſed to the open air, the ſurface of the liquor exhibits a party coloured pellicle, compoſed of particles of iron, reduced to ſuch tenuity as to reflect various colours; by degrees, all the iron is ſeparated in form of a ſubtile ochre, amounting to nearly $\frac{1}{10500}$ part of the weight of the water, and, when freſh, retains as much of the principle of inflammability, as to obey the magnet, and be ſoluble in mineral acids. Cauſtic alkali, whether fixed or volatile, ſeparates this metal from the water; but, when perfectly ſaturated with fixed air, they have no effect.

Zinc diſſolves copiouſly in aerated water. The metal is precipitated of a yellowiſh aſh colour by alkali, when perfectly cauſtic; and alſo by tincture of galls, and phlogiſticated alkali. Alkalis fully aerated have no effect.

He found aerated water diſſolve the black calx of manganeſe, but much more copiouſly the regulus of that metal. Phlogiſticated alkali, and tincture of galls, precipitate pure manganeſe,

ganeſe, of a yellowiſh white. When the regulus is employed, the ſolution diffuſes a particular ſmell like burnt fat.

He found little effect from the aerial acid on the other metals.

Fixed air, he ſays, has a great tendency to unite with inflammable matters. Spirit of wine, in the temperature of 50 of Fahrenheit, abſorbs twice its own bulk. Olive oil, its own bulk, and ſometimes more. Oil of turpentine, twice its own bulk, and that with ſuch avidity, that, in the firſt half hour, about a fourth part of the air diſappears. Æther expands it to twice its bulk; but, on paſſing the vapour through water, it is again reduced to its original quantity. If the fixed air contained in olive oil be again expelled by fire, it becomes capable of ſuſtaining flame, and is almoſt inſoluble in water.

He found fixed air with difficulty combined with phlogiſton, although it appeared united in charcoal. He ſays, fixed air may be loaded with phlogiſton, by drawing electrical ſparks through it, ſo as to make it reject an union with water.

He remarks, that oils do not unite with alkalis, unleſs they are perfectly cauſtic; but if deliqueſcent alkali be poured into oil, and remain unſhaken, very fine cryſtals will be formed at the bottom. They form, he ſays, equally well in unctuous oils or oil of turpentine, but beſt of all in animal oils.

He obſerves, that as the rancidity of oils depend on the want of fixed air, which forms a common part of oil, the alteration may be prevented or cured, by reſtoring as much fixed air as the oil had loſt. M. Sieffert has given a good proceſs for reſtoring oil to mildneſs, by mixing with it $\frac{1}{10}$ of apples, plumbs or ſtrawberries, reduced into a pulp, and ſetting it to ferment, aſſiſting it with a little honey. The oil drawn off by this proceſs, he ſays, will not only be reſtored, but alſo improved.

He proves, that fixed air, like other acids, precipitates bodies diſſolved in pure alkalis; thus, ſulphur diſſolved in alkalis, or in lime-water, is precipitated by the addition of fixed air. Liquor of flints in the open air gradually depoſits its ſiliceous earth; but much ſooner, if expoſed to fixed air. This is the reaſon why a ſolution of alkali of tartar, though repeatedly filtered, continues for a great length of time to depoſit earthy flocculi; for this alkali contains in it flinty particles, acquired either during vegetation or combuſtion. The calciners of potaſhes ſometimes fraudulently add ſand, in order to increaſe the weight, which, during the operation, ſo unites with the aſhes, that the flinty matter, by means of the alkaline ſalts, becomes ſoluble in water along with them. This flinty matter afterwards gradually ſeparates in proportion as the alkali is ſaturated with fixed air, with which the latter more readily unites. As the alkali is generally kept in a narrow necked bottle, commonly ſtopped, it muſt receive the precipitating matter from the atmoſphere very ſlowly; the ſeparation, therefore, proceeds very gradually: but if it be diſſolved in a large quantity of water, containing fixed air, or if, from the ſolution having been long made, it is ſufficiently loaded with fixed air, the whole earthy matter falls at once to the bottom.

He found ſoap decompoſed with difficulty by fixed air, partly by reaſon of the weakneſs of the acid, and partly by the alkali ſuperabounding.

He next examines the ſimple elective attractions of fixed air. He found terra ponderoſa retain fixed air more ſtrongly than pure vegetable alkali; and that 100 parts of aerated vegetable alkali contains as much air as is neceſſary to ſaturate 390 parts of pure terra ponderoſa: but 100 parts of aerated alkali ſaturated no more than 68 of pure lime. 263 parts of mineral alkali ſaturated 100 of vegetable alkali. 93 parts of aerated volatile alkali gives fixed air to 100 of pure vegetable alkali, and aſſumes a cauſtic nature. 168 parts of aerated magneſia ſaturates only 100 of pure vegetable alkali.

Volatile alkali gives its fixed air to terra ponderoſa, lime, fixed alkali, and magneſia. This laſt gives its fixed air to terra ponderoſa.

Terra ponderoſa, diſſolved by a ſuperabundant quantity of fixed air, is precipitated by all alkalis, pure lime, and magneſia; theſe taking up the ſuperabundant aerial acid, by which the earth is merely ſaturated, in which ſtate it is indiſſoluble.

Common magneſia is precipitated from water, by all alkalis, terra ponderoſa, and lime.

Metallic bodies, diſſolved by means of fixed air, are precipitated by alkalis or earths, when cauſtic.

From the foregoing data, he forms the following table of elective attractions:

Aerial Acid
Pure terra ponderoſa
—— lime
—— fixed vegetable alkali
—— mineral alkali
—— magneſia
—— volatile alkali
Zinc
Manganeſe
Iron.

With reſpect to acids, our author conſiders the aerial to be the weakeſt; for it is expelled not only by vinegar, but alſo by the phlogiſticated acids of nitre and vitriol.

Pure volatile alkali, he found, would precipitate neither terra ponderoſa, lime, nor magneſia, diſſolved in any common acid; but when aerated, it will effect a precipitation very readily. Here the pure alkali is the ſtrongeſt, yet it has a leſs attraction for acids than lime; but in the caſe of aerated alkali, when added to a ſolution of calcareous ſalt, the ſalt is acted upon by a double force; one, the attraction of the pure alkali ſoliciting the acid; the other, that of the fixed air for the lime. Theſe forces joined together overcome the original adheſion of the lime and acid. The ſame is the caſe with the precipitation of metals. Here a remarkable difference of weight of the precipitate takes place. When the aerated alkali is uſed, mercury gains one-fourth more than when pure alkali is made uſe of; iron nearly an equal weight; and ſilver in the ſame proportion. Theſe acceſſions of weight are attributed to fixed air. Being expelled, it adheres to the deſerted metal, which, according to its quantity, alters not only its weight, but alſo the colour. This pure vegetable alkali precipitates corroſive ſublimate of a ferruginous colour; but when fully aerated, the precipitate is white.

Our author next proceeds to the different proofs, that fixed air is a true acid. In common with other acids, he ſays, it impreſſes on the tongue a ſour taſte, reddens tincture of turnſol, attacks fixed alkalis, and renders them mild. He finds, that a ſmaller quantity of this acid than of the ſtronger ones ſaturates them, renders them cryſtallizable, and leſs ſoluble. By its union, it renders volatile alkalis more fixed, leſs odorous and penetrating, and cryſtallizable.

lizable. When lime is juſt ſaturated with fixed air, it is rendered inſoluble, more mild and cryſtallizable; but when ſuperabundant, it renders it again ſoluble. It produces the ſame effects with terra ponderoſa; and with magneſia it forms a neutral cryſtallizable earthy ſalt. With iron, zinc, and manganeſe, it forms ſalts, which, when diſſolved in water, redden tincture of turnſol, like all other metallic ſalts. It exerts both ſimple and double elective attractions, like other acids. It precipitates ſubſtances diſſolved in pure alkalis. It is with difficulty detached from water, even by boiling; but congelation ſeparates it entirely. And, laſtly, it greedily attracts phlogiſticated matters.

From the foregoing circumſtances, he thinks he has fully aſcertained fixed air to be an acid; and, from the common atmoſphere being impregnated with it, he applies the name atmoſpheric or aerial acid to it. The preſence of this acid every where appears from its effects. Lime-water, by means of it, has a cream or cruſt formed on its ſurface. Quicklime aſſumes the nature of crude lime, and becomes unfit for the maſon's uſe. Pure magneſia and terra ponderoſa recover their weight, power of

effervеſcence,

effervefcence, and other qualities, by being expoſed to the open air. Pure alkalis are made mild in the open air, loſe their deliquating quality, and are diſpoſed to form cryſtals which effervefce with acids.

He ſays that fixed air, not only in an elaſtic and diſengaged ſtate ſurrounds us, but alſo conſtitutes a primary part of common air, which he proves to conſiſt of three elaſtic fluids mixed together, *viz.* 1*ſt*, The aerial acid in its diſengaged ſtate, but in ſmall quantity: 2*dly*, Vitiated or phlogiſticated air unfit for ſuſtaining flame; and, 3*dly*, Pure air, indiſpenſably neceſſary for ſuſtaining flame, and animal life, which, he ſays, forms about one-fourth of the whole.

He next endeavours to prove, that the aerial acid is a pure acid *ſui generis*, and not produced by any foreign acid. In proof of this, he obſerves, that if lime-water be ſaturated with this acid, a precipitate is formed, which is the ſame with cream of lime or crude lime. 2*dly*, If the acidity depended on foreign admixture, different kinds of fixed air would be produced from different acids; but experience ſhews, that fixed air is the ſame by whatever acid it is produced. 3*dly*, The fixed air, obtained by means of acids, is the ſame with that collected from fermenting liquors. *4thly*, Calcareous ſpar and magneſia afford genuine fixed air, purely by the force of fire; and that even after being minutely powdered and boiled for half an hour in pure alkaline lixivium, and afterwards waſhed and dried before they are put into the retort.

From all theſe circumſtances, he concludes, that all ſuſpicion of a foreign acid vaniſhes, and that the aerial acid is an acid *ſui generis*.

He next endeavours to aſcertain the ſpecific gravity of fixed air, in the ſame way as that of common air. He found fixed air, compared with an equal bulk of common air, to be 0,0006 heavier. From this ſuperior gravity, it follows, that the lower ſtrata of the atmoſphere muſt abound more with fixed air than the higher; and hence he thinks it is plain, why higher ſituations are in general more healthy than the lower ones: For he has no doubt, that various diſorders, both epidemic and endemic, ariſe from different quantities of aerial acid in the atmoſphere.

He ſays fixed air not only prevents fire from being kindled, ſo as to hinder the exploſion of bombs and fire arms; but that it alſo completely and inſtantaneouſly extinguiſhes a body red hot and flaming.

This acid, he ſays, very ſtrongly attracts ſmoke, extends it in a parallel to its own ſurface, and holds it a long time.

In experiments made on animals, he uſes a glaſs bell, with a wooden bottom, to which the bell applies cloſely. At the top of the bell is a ſmall hole, and through the wooden bottom a crooked tube is paſſed. The hole through which the tube paſſes fits ſo exactly as to allow no air to be tranſmitted. The animal is put into the bell, which is cloſely applied to the board. The fixed air is then introduced through the tube, which expels the lighter common air through the hole at the top of the bell. At firſt they are, however, in ſome degree mixed; hence the animal does not die ſo ſoon as when immerſed directly into fixed air.

As the noxious vapour ruſhes through the tube, the animal looks about with great anxiety, in order to eſcape. It then begins to pant; the eye-balls are protruded; it trembles, and at length expires, as if going to ſleep. By regulating the influx of aerial acid, the approach of death, he ſays, may, at pleaſure, be protracted.

After death, he found the lungs appear ſomewhat collapſed. They do not ſink in water like thoſe of an animal that has periſhed in vacuo, and are in many places inflamed: the trunk of the pulmonary artery, right ventricle of the heart, with its auricle, the vena cava, jugular veins, and veſſels of the brain, are diſtended with blood; and he often found a firm polypus in the right ventricle: the pulmonary veins, aorta, and left ventricle of the heart, with its auricle, are generally flaccid: the irritability of the muſcular fibres all over the body, he found deſtroyed ſo far, as not to be excited by any irritation whatever, even though taken out warm. He found, with reſpect to different ſpecies of animals, that their age and vigour made ſome difference in the effects. Birds, in general, die ſooner than dogs. Amphibious animals endure longer; and inſects longeſt of all. The younger animals, eſpecially ſuch as are accuſtomed to this air, he found, were longer of being deſtroyed.

Upon the whole, our author thinks he has eſtabliſhed that fixed air is an acid *ſui generis*, the ſame with the damps in coal-pits, and other caverns of the earth; that it may be collected from alkaline ſalts and earths by ſtronger acids, and by fire, and from different vegetables under fermentation; that it is found floating in the atmoſphere, of which it makes a part; that when diſengaged, it is inimical to animal life, and to flame; and that it gives ſapidity, and the ſparkling appearance to the cold medicated ſprings known by the name of acidulæ.

IV.

Jani Peterſen Michell, *M. D. De Synchondrotomia Pubis Commentarius.* 8vo. Amſtelodami.

THIS treatiſe was firſt, we are told, publiſhed under the form of an inaugural diſſertation. Since that it has been reprinted in a more extenſive form, with many additional obſervations, and illuſtrated by different plates. As from the importance of the ſubject, as well as the merit of the work, it is juſtly entitled to the attention of the public, we ſhall here preſent

ſent our readers with an analyſis of it extracted from the Leipſic Commentaries.

This diſſertation is divided into three ſections. In the firſt of theſe, the author conſiders the invention and progreſs of this operation. And here he takes occaſion to obſerve, that it had been ſuggeſted by Pinæus, Cordæus, Veſalius, Petit, and others prior to Mr Sigault, who is, however, unqueſtionably entitled to the praiſe of having firſt performed it on a living woman, on the firſt of October 1777. After theſe obſervations on the invention of this operation, he next gives an accurate enumeration of all the writers who have treated of this ſubject; both thoſe who have recommended, and thoſe who have condemned it, mentioning thoſe authors in a chronological order.

In his ſecond ſection he enquires into the utility of this operation, in caſes of difficult labour; and firſt, he enquires, whether, from a natural dilatation of the bones of the pelvis, this operation can be conſidered as pointed out to us. Here alſo he mentions the different authors who have either aſſumed or denied this dilatation; and after attempting to refute the opinions of ſome of them, he contends that the pelvis is actually dilated in the time of labour. Nor does he believe that this dilatation is ſolely effected by the head acting like a wedge; but he rather aſcribes it to the operation of the fluids ſeparated at the pelvis. But he concludes with obſerving, that Nature employs this remedy in thoſe caſes only where delivery is otherwiſe impoſſible, and that this dilatation of the pelvis is never to be wiſhed for; but that, on the contrary, delivery is to be promoted by art, leaſt it ſhould take place.

He next conſiders whether the ſtructure of the pelvis affords any objection to this operation, and whether it can be performed without danger to the woman in labour. This leads him to conſider the natural connection of the bones, and to point out inſtances where an anchyloſis has taken place, both of the bones at the pubes, and likewiſe of the os ſacrum with the oſſa ilia. Hence he takes occaſion to mention that the ſtructure of the bones may be ſuch as to require the Cæſarian operation; and he concludes with obſerving, that the ſection at the pelvis can give very little dilatation, while, at the ſame time, from this operation, a leſion and dilaceration of ligaments may ariſe.

In the third chapter of this ſection, he conſiders whether the operations hitherto performed militate in favour of this operation. And here he enters into a relation of all the operations of this nature which have been performed and publiſhed by Meſſrs Sigault, le Roi, de Cambon, van Munter, and others. And from theſe he endeavours to ſhew, either that the operation was not neceſſary, or that it was attended with the worſt conſequences.

The ſubject of the fourth chapter is an enquiry how far experiments made upon dead bodies and upon brutes can afford any proof of its utility in caſes of difficult labour. Having demonſtrated the difference which takes place in this reſpect between the human ſpecies and other animals, and between dead and living bodies, he relates the experiments of Camper Plenk, and others, as well as thoſe performed by himſelf: From all which he concludes, that ſynchondrotomy cannot ſupply the place of the Cæſarian operation, where the cauſe of narrowneſs in the pelvis is placed in its ſtraight diameter.

In the fifth chapter of this ſection, he enquires whether any caſes can be conceived to which ſynchondrotomy is ſuited, and where it can ſupply the place of the Cæſarian operation. He therefore conſiders the ſcope of the operation by which the ſection at the pubes is made, the caſes in which it has been propoſed by authors, and the evils which reſult from it, either to the mother or her offspring. And he concludes, that this operation is to be had recourſe to, when the mal-conformation is in the tranſverſe diameter of the inferior aperture of the pelvis, or when the head of the fœtus is ſo wedged in at the inferior part of the pelvis, that it is neither poſſible to extract it by the forceps, nor even to draw it back into the uterus, ſuppoſing the Cæſarian ſection to be performed. But he conſiders this operation as being in every caſe a doubtful remedy.

In the laſt ſection of this work, the author treats of the Cæſarian operation or hyſterotomy, as it is more properly called. Here, in three chapters, he endeavours to ſhew in what caſes of difficult labour it is proper, and enquires whether the danger to the mother and fœtus be ſo great as ſome have alleged. And he obſerves, that it can by no means be conſidered as always fatal.

The authors of the Leipſic Commentaries conclude their analyſis of this work, by giving it as their opinion, that the ſubject of which it treats, is here ſo fully and accurately examined, as ſcarce to leave room for adding any thing more reſpecting it, and they are inclined to think, that, by this treatiſe, all the diſputes which have taken place about this operation may be conſidered as finally ſettled.

V.

Expériences ſur la Reſpiration des Animaux, et ſur les changemens qui arrivent à l'air en paſſant par leur Poumon. Par M. Lavoiſier. Vide *Hiſtoire de l'Académie Royale des Sciences, année* 1777. 8vo. Paris 1780.

OF all the phænomena exhibited by the human body, none are more ſtriking or more worthy of the attention of philoſophers or phyſicians than thoſe which attend reſpiration. If, on the one hand, we know little of the object of this ſingular function, on the other, we know that it is ſo eſſential to life, that it cannot be ſuſpended even for a ſhort time, without expoſing the animal to danger of approaching death.

While it is well known that common air is the ſubject of reſpiration, it is no leſs certain, that there are a great number of airs which animals cannot reſpire, without periſhing as quickly, at leaſt, as if they did not breath at all.

After taking notice of the diſcoveries made by Dr Hales and Mr Cigna, our author obſerves, that Dr Prieſtley had thrown great light upon this ſubject, by a ſeries of experiments equally new and ingenious, from which he endeavoured to prove, that the reſpiration of animals had the property of phlogiſticating the air in the ſame manner as the calcination of metals and ſeveral other chemical proceſſes, and that it did not ceaſe to be reſpirable till it was ſurcharged, and as it were ſaturated with phlogiſton.

Mr Lavoiſier obſerves, that however probable the opinion of the celebrated philoſopher may, at firſt ſight, appear, the reſult of his own experiments led him to a very different concluſion. Without entering, however, into a particular examination of thoſe of Dr Prieſtley, he proceeds, in the memoir before us, to relate his own experiments, and to give an account of the reſult of them.

In calcining mercury with a given portion of atmoſpheric air, proper for reſpiration, one part of this air united with the mercury; the remainder ceaſed to be fit for reſpiration or combuſtion. Nitrous air did not produce any diminution of it; but this air did not render lime-water turbid.

If, by a violent fire, applied to calcined mercury, the air which it contains be ſeparated from it, and thus mixed with the reſiduum, not reſpirable, mentioned above, it reſtores to it all its former properties.

If an animal be placed under a bell-glaſs in atmoſpheric air, it periſhes in a ſhort time; the air confined in the glaſs is found to be diminiſhed, but leſs than that in which mercury has been calcined. That which remains is not reſpirable, it extinguiſhes flame, and is not farther diminiſhed by nitrous air; but he found that this air, contrary to what has been the caſe with that affected by the calcination of mercury, rendered lime-water turbid. In this particular, therefore, as well as in its being leſs diminiſhed in bulk, there was an eſſential difference between the two.

When alkali in a cauſtic ſtate was placed in this air, affected by animal reſpiration, a part of the alkali loſt its cauſticity, and it abſorbed a portion of the air. The reſiduum thus formed, conſiſting of air, deprived, in the firſt place, by animal reſpiration of the vital air which it contained, and afterwards by the cauſtic alkali of the fixed air, or more properly the gaſſeous air which it contained, Mr Lavoiſier found to differ in no reſpect from the air affected by the calcination of mercury. And he found alſo, that, by the addition of the air detached from calcined mercury, it was again rendered capable of ſupporting life. Hence he concludes, that while the calcination of mercury produces no other change on atmoſpheric air than that of depriving it of the vital air, if it may be ſo called, which it contains, the reſpiration of animals, beſides this effect, produces in the atmoſpheric air a portion of gaſſeous or fixed air.

Here a queſtion, he thinks, may occur, *viz.* whether a portion of the air, peculiarly fitted for reſpiration, and which may therefore be ſtyled vital air, be converted into gaſſeous air in paſſing through the lungs; or whether in this viſcus there takes place, as it were, an exchange, on the one hand the vital air being abſorbed, and, on the other, the lungs reſtoring, in place of it, a portion of the aeriform acid of chalk, or what has been commonly, though very improperly, called fixed air, nearly equal in bulk to the portion of vital air abſorbed? On this ſubject he obſerves, that from his own experiments, and thoſe of Dr Prieſtley, vital air is neceſſary for preſerving the colour of the blood; that it loſes its colour in other kinds of air, and even in vacuo. From this he thinks it may be inferred, that there is a real abſorption of vital air. But, on the other hand, it is certain, that vital air, in which charcoal has been burnt, has the appearance of being transformed into gaſſeous air. This again he thinks ſeems to ſhew, that vital air may, by reſpiration, be transformed into gaſſeous air. He is in ſome degree diſpoſed to adopt both opinions, and to believe, that during reſpiration there takes place, at the ſame time, both an abſorption of part of the vital air, and a converſion of another part of it into gaſſeous or fixed air.

VI.

Mémoire ſur la néceſſité de faire l'operation Ceſarienne aux femmes qui meurent enceintes, et ſur les moyens de rapeller leurs enfans d'une mort apparente à la vie. Par M. Bordenave. Vide *Hiſtoire de l'Académie Royale des Sciences, année* 1777. 4to. Paris.

IN the memoir before us, Mr Bordenave ſets out with obſerving, that if there be an infinite number of diſeaſes to which mankind are expoſed, unavoidably terminating in death, by deſtroying the ſources of life, ſo there are often other external cauſes, which, acting differently on the organs, ſuſpend only the vital action, but in this manner produce an apparent death, which will ſoon become real, if proper aſſiſtance be not given. This is exemplified both in the caſe of perſons ſuffocated by the ſmoke of charcoal, and in the caſe alſo of many new born infants.

Theſe, by proper aſſiſtance, are often recovered; and our author is of opinion, that art may here extend its influence ſtill farther to children, *viz.* periſhing in the womb, who have ſurvived the death of the mother. This aſſertion, in the memoir before us, he attempts to ſupport by a collection of facts, which taken ſingly might not be ſuppoſed to have much weight.

The firſt caſe he mentions, is that of a woman who died about an hour after mid-day, and was opened about ſeven in the evening by M. Dayries, ſurgeon to the Hotel-Dieu. She was ſuppoſed to have been only ſix months gone with child. He, however, took from her womb a male child, diſtinct in all its parts and, by the redneſs of its colour, ſhewing the appearance of life. A palpitation was diſtinctly felt at the region of the heart. Reſpiration ſoon began, and it continued to live for two hours.

After this, Mr Bordenave takes notice of a number of caſes mentioned by Mr Cangiamila, in his Embryologia Sacra, a work which he repreſents as highly valuable, and too little known. On that authority, he remarks, that, in the city of Montreal and the neighbourhood, in the ſpace of twenty-four years, twenty-one living children had been extracted by the Cæſarian operation: that between the year 1704 and 1748, ſixty children had been extracted in the ſame manner at Caltaniſſecta, of whom only five were found dead: that at Victoria, a city in the dioceſe of Syracuſe, between the years 1734 and 1752, the Cæſarian operation had been performed twenty times, and in every caſe a living child extracted: and that at Sambuca, a city in the dioceſe of Girenti, twenty-two pregnant women having died, from eighteen of them children were extracted alive; in three of the caſes, from the putrefaction which had taken place, there was reaſon to believe that the child had been dead ſome time before the death of the mother; but the fourth was found ſtifled among the bed-clothes, probably having come into the world among the laſt moments of the life of the mother. Where, therefore, a pregnant woman dies, eſpecially in the advanced periods of that ſtate, the Cæſarian operation ſhould never, he thinks, be neglected, but ought to be regarded as an act of humanity.

This operation may, he obſerves, in many caſes, particularly where there is ſudden death, be delayed too long, from an apprehenſion that the mother herſelf may be ſtill in a recoverable ſtate. But, excluſive of the other ſigns of death, this, our author thinks, may be certainly determined by the inſpection of the eyes alone, which, in a ſhort time after death, acquire a dull and collapſed appearance. But beſides this, he alſo propoſes, that, in every caſe, the ſame attention ſhould be paid as would be done with a living woman, where the aim was to preſerve both mother and child.

But the performance of the operation, he obſerves, is not all. It is neceſſary to diſtinguiſh whether the child brought into the world without apparent ſigns of life, be really in a recoverable ſtate or not, that proper meaſures may, in the former caſe, be taken for its revival. Life, he obſerves, is more eaſily retained in an infant which has never breathed, than after this function has been exerciſed, as circulation can then go on without the aid of reſpiration. Paleneſs of the body, lividity, coldneſs, the want of reſpiration, and even of obvious pulſation, are not then certain marks of death. Putrefaction alone, he thinks, affords an indubitable ſign;

and even this is to be carefully diſtinguiſhed from a gangrenous diſpoſition, which may be the effect of compreſſion, contuſion, or ſimilar accidents.

For the recovery of infants apparently dead, he recommends the blowing warm air into the mouth, irritation of the noſtrils, bathing in aromatic decoctions, the application of compreſſes dipped in warm wine, ſuction of the niples, particularly the left, which he thinks is peculiarly ſenſible from its numerous nerves, and, finally, injection of tobacco-ſmoke into the inteſtines. Theſe meaſures are, he thinks, particularly proper where death is the conſequence of weakneſs, or a ſtagnation of the fluids. But when it ariſes from a ſuffocation of the vital principle, occaſioned by plethora, or by that obſtruction which has ariſen from compreſſion of the infant, then a ſlight evacuation of blood, by cutting the umbilical cord, becomes, he thinks, uſeful, and ſhould precede other practices. Theſe meaſures are not always immediately followed by the deſired effect. From experience, it appears, that, in ſome inſtances, they ſucceed only after being tried for ſeveral hours. And he particularly records one inſtance publiſhed in the Gazette de Manheim, for the year 1773, where a recovery was effected by long continued perſeverance, after the moſt probable meaſures had been uſed for a conſiderable time, without any proſpect of ſucceſs. To theſe he ſubjoins two other ſimilar caſes, the one by Mr Andrieu, ſurgeon at Gaillac, the other by Mr Faiſſolis, ſurgeon at Lyons. And he concludes with obſerving, that, from theſe facts, it appears, a laudable attention to infants remaining in the womb of thoſe mothers who die in a pregnant ſtate cannot be too much encouraged. Though the labour ſhould often be in vain, yet it will be an ample recompence, if, in ſome inſtances, the life of a child can be preſerved.

VII.

Mémoire ſur une ſubſtance aëriforme qui émane du corps humain, et ſur la maniere de la recueillir. Par M. le Comte de Milly. Vide *Hiſtoire de l'Académie Royale des Sciences, année* 1777. 4to. Paris.

IN the memoir now before us, M. Milly ſets out with obſerving, that the perſpiration of living bodies had been obſerved as early as the days of Heraclitus, of Hippocrates, and of Ariſtotle; but that Sanclorius was the firſt who made any deciſive experiments upon this ſubject, and that it has ſince been very much laboured by Keil, de Gorter, Linning, Chalmers, and others. From their trials, it appears, that the emanation from living bodies is much varied by many different circumſtances, particularly by the kind and quantity of the aliment employed, by the ſtate of the atmoſphere, by the natural heat of the individual on whom the experiments are made, and, finally, by the temperature of the climate; for the diſcharge is much leſs in winter than in ſummer, and in cold than in warm countries.

It is here neceſſary to obſerve, that the obſervations offered by our author on this ſubject, do not, in any degree, reſpect pulmonary perſpiration; he means only to treat of that aeriform diſcharge which takes place by the ſurface of the body, and which he conſiders as eſſentially different from the liquid which exhales from the pores, and is known by the name of ſweat. The one is ſo different from the other, that, when ſweating takes place, he conſiders aëriform perſpiration is inſtantly ceaſing.

The purpoſe of the preſent memoir, is to give an account of the circumſtances which led him to obſerve this ſingular fluid; to point out the manner in which he has been able to collect it, and by which it may be collected by others in ſufficient quantities for ſubmitting it to experiments, from which its nature may be inveſtigated; and to preſent to the learned, a ſubject yet unexplored, on which they may, in ſome reſpects, extend the boundaries of our knowledge of the animal œconomy. The philoſopher, he obſerves, may now ſee with his own eyes, the remarkable ſhare which a peculiar air has in the animal œconomy, and the chemiſts may ſubmit this unknown ſubſtance to experiments for diſtinguiſhing its nature.

It has been the obſervation, he remarks, of a celebrated modern, that the ennui may even contribute to our knowledge. In this ſtate, he had occaſion to obſerve, in the bath whilſt he lay at reſt, a prodigious quantity of ſmall ſilver coloured air-bubbles, reſembling what are called the ſeeds of pearls, on the ſurface of his body. In a ſhort time they became larger; and, at laſt, on the ſlighteſt agitation, detached themſelves from their baſe, roſe to the ſurface of the water, burſt, and diſappeared.

Struck by this intereſting appearance, he formed the deſign of collecting this aeriform ſubſtance. A glaſs decanter, which ſerved for holding lemonade, and which he had by him, was the firſt inſtrument which he employed. This he filled with water, and having inverted it, he held the mouth of it above thoſe parts where the air-bubbles were moſt numerous. He then gently touched the air-bubbles with his other hand. Theſe inſtantly roſe to the ſurface of the water contained in the inverted carafe. From this firſt eſſay he had demonſtration, that this aeriform fluid might be thus, without much difficulty, collected.

He returned the ſucceeding day, provided with a better apparatus; a bottle of conſiderable ſize, and a funnel about eight inches in diameter. He then went into the warm bath, and after remaining at reſt four minutes, he perceived the air-bubbles begin to form. Then, with his left hand, he held over them his bottle filled with water, and inverted, to the mouth of which the funnel was properly adapted. After this, with his right hand, he gently rubbed the ſkin to make the bubbles riſe. Theſe mounted up very quickly, like particles of oil, and ſoon aſſembled in the inverted funnel, from which they paſſed into the bottle, and diſplaced the water as they roſe.

This method, our author obſerves, is ſo eaſy, that, in two hours, as much may be collected as will fill an half pint bottle; and any one has it in his power to repeat theſe trials, without any other apparatus than that of an ordinary bath.

It remains then, he remarks, to ſubmit this animal air to proper experiments; and particularly, in the *firſt* place, to determine its ſpecific gravity,

gravity comparatively with diſtilled water, and with atmoſpheric air; and, 2*dly*, to diſcover whether it has any thing in common with the different gaſſes obſerved by modern philoſophers. In the courſe of theſe experiments, we may, perhaps, ſays he, diſcover the reaſon why a great number of people, aſſembled in a confined ſituation, infect the air in ſuch a manner, as to render it unfit for reſpiration. It is, he thinks, not improbable, that this matter which conſtantly departs from living bodies, and which nature, as it were, rejects, may communicate to atmoſpheric air thoſe bad qualities which render it mephitic. But he concludes with obſerving, that it is experience alone which can unfold theſe myſteries. This ſubject the Count himſelf has, with equal ingenuity and induſtry, proſecuted, at leaſt ſome length, as will appear from the analyſis ſubjoined.

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Second Mémoire ſur le Gas Animal, par M. le Comte de Milly.

In this memoir, which is contained in the ſame volume with the former, Comte de Milly ſets out with obſerving, that he had neither had ſufficient time, nor been able to obtain a ſufficient quantity of this animal gas, as he ſtyles it, for properly determining its nature; but that he had been able to aſcertain ſome points, and to ſettle ſome doubts, that had been ſuggeſted to him reſpecting it. An account of theſe, then, is the ſubject of the preſent memoir.

It had been ſaid, among other things, that the ſubſtance which he had collected, might be merely common air, attached to the ſurface of the ſkin, which on being rarefied by the heat of the bath, was again detached from thence. But that it was really an emanation from the body, ſeems to be clearly proved by the following experiments which he made in conjunction with Mr Lavoiſier.

1. They filled with this animal air, a cylindrical glaſs veſſel, and immerſed into it a lighted candle, which was immediately extinguiſhed.

2. They mixed this animal gas with lime-water, and inſtantly the lime-water became milky, and was ſubjected to a precipitation.

3. They put four parts of nitrous air into a glaſs cylinder properly graduated, and they added

ded to it two parts of animal gas. This mixture occaſioned almoſt no red vapours; and the little which did appear, was, they thought, to be attributed to a ſmall quantity of common air which was mixed with the animal gas, in its paſſage from one veſſel into another.

The want of animal gas prevented them from carrying their experiments farther; but thoſe above enumerated are ſufficient to prove, that this ſubſtance is not common air, but that it poſſeſſes the properties of what has commonly been called fixed air, and that it appears to be of the ſame nature with pulmonary air.

Theſe two emanations from the animal body being of the ſame nature with fixed air, in which animals periſh, the reaſon is, he obſerves, very plain, why, when a number of perſons are aſſembled in a confined ſituation, where the air is with difficulty renewed, many of them ſhould be taken ill, and even the whole would periſh, if time was given for this emanation to be collected.

It is not then from the loſs of elaſticity, as has commonly been ſuppoſed, but from a mephitic quality, that the inſalubrity of theatres, churches, and other crowded places of public reſort, ariſes. And the cauſe of the evil being known, it will not be difficult to remedy it. This may be moſt readily and efficaciouſly accompliſhed by a quick renewal of the air. And in the conſtruction of public buildings, a due regard to this ſhould claim the attention of every architect, as being a circumſtance on which the health and life of numbers muſt very much depend.

VIII.

Mémoire ſur quelques maladies du Foie qu'on attribue à d'autres organes, et ſur des maladies dont on fixe ordinairement le Siége dans le Foie, quoiqu'il n'y ſoit pas. Par M. Portal. Vide *Mémoires de l'Académie Royale des Sciences, année* 1777. 4to. Paris.

OF all the diſeaſes which afflict mankind, none are more dangerous than thoſe of which we are ignorant of the ſeat. Every viſcus has a texture peculiar to itſelf; it is ſubject to affections from which the others are free; and it is freed from theſe in a manner peculiar to itſelf. Since, then, the great art of medicine is to aſſiſt nature, this can be done only when the ſeat of the diſeaſe is perfectly known. This conſideration has induced our author to endeavour to arrive at the diſcovery of truth, by the examination of dead bodies. And anatomy, it muſt be allowed, is in no reſpect more uſeful in medicine, than in pointing out to us the morbid conditions to which parts are ſubjected.

In the memoir now before us, M. Portal, availing himſelf of the information thus acquired, endeavours to point out errors reſpecting diſeaſes of the liver, which his practice has diſcovered to him. And here he is not aſhamed to own, that he has often been miſtaken; a confeſſion which merits much higher praiſe than pretenſions to infallability. For, from an acknowledgment of error, an author obtains the merit of integrity, the advantage of being uſeful, and the honour of having himſelf diſcovered truths which had formerly eſcaped his obſervation.

He ſets out with obſerving, that effuſions into the right cavity of the cheſt, and enlargements of theſe lobes of the lungs which it contains,

tains, often cauſe ſuch a derangement in the ſituation of the liver, that it is ſuſpected to be obſtructed, although in a perfectly natural ſtate.

In confirmation of this aſſertion, he relates the caſe of an advocate, whoſe ordinary phyſician, from the ſymptoms, concluded that he was ſubjected to a diſeaſe of the liver. Mr Portal, when called in conſultation, diſcovered a large tumour under the falſe ribs of the right ſide. And as, while the ſymptoms to which he was principally ſubjected, loſs of ſtrength and appetite, accompanied with yellowneſs of the ſkin, and the like, indicated an affection of that organ, as there was no cough, pain of heart, purulent expectoration, or any other circumſtance which pointed out an affection of the lungs, his opinion perfectly coincided with that of the ordinary phyſician. On this idea, then, they directed their practice; but the diſeaſe increaſed daily, and a colliquative diarrhœa ſupervening, the patient was cut off by a maraſmus.

Mr Portal was ſo confident of the diſeaſe of this patient, that he would not have thought of opening the body, had it not been his practice to open every patient who died under his care. But how great, ſays he, was my ſurpriſe, when I found the liver in a perfectly natural ſtate. It was in the lungs alone that the diſeaſe had its ſeat. Theſe were full of obſtructions formed by a ſcrophulous ſubſtance. And in the right ſide there were ſeveral abſceſſes communicating together, from whence was diſcharged a conſiderable quantity of purulent matter. The volume of the lungs thus increaſed, depreſſed the diaphragm, and the liver was conſequently puſhed lower than its natural ſituation. In this manner it formed, below the falſe ribs of the right ſide, that protuberance which was taken for a large obſtruction.

After this, Mr Portal mentions, ſeveral ſimilar caſes, particularly that of the late Duc de Chaulnes, treated by him and by M. Bordeu. He remarks alſo, that a ſimilar deception may take place on the left ſide, from the ſpleen being diſplaced in the ſame manner. But as its ſize is leſs, the enlargement is never ſo great.

Another kind of miſtake often committed, is that of believing that there is a diſeaſe of the liver, wherever yellowneſs occurs as a ſymptom. It is true, he remarks, that this colour is obſervable in many affections of the liver. But as there are ſome diſeaſes of that viſcus which are followed by no yellowneſs, ſo, on the other hand, yellowneſs often ſupervenes, where the liver is perfectly ſound, and where the ſecretion of bile takes place in the moſt regular manner. He tells us, that, in ſeveral inſtances, upon opening the bodies of patients, who died from obſtructions in the inteſtinal canal, and in whom a yellow colour had been obſerved before death, he has found the lacteal veſſels filled with a yellow fluid, bitter to the taſte, and capable of inflammation when thrown upon the fire. Hence he concludes, that when the bile is obſtructed in the inteſtine, it makes its way into the lacteals, and is thus inſinuated into the blood, though the ſecretion be perfectly natural. In the ſame manner alſo, he explains the yellow appearance in new born infants. For he contends, that he has found a ſimilar fluid, not only in their lacteal veſſels, but in the thoracic duct: that, in theſe caſes, he has found the gall-bladder almoſt empty: that, when the gall-bladder, on the other hand, has been much diſtended, there has been no jaundice: and, finally, that this yellow colour does not take place where the meconium is freely diſcharged, and where, therefore, no obſtruction happens in the inteſtinal canal.

From theſe circumſtances, he concludes, that yellowneſs, in ſome caſes, takes place without any affection of the liver, but from a reflux of bile into the blood, through the lacteal veſſels. And, in further proof of this doctrine, he tells us, that, in dogs, he has induced jaundice by putting a ligature on the ſmall inteſtines.

If, however, on ſome occaſions, diſeaſes have been attributed to the liver, where it is not affected; ſo, in other caſes, diſeaſes have had their ſeat in this viſcus, where the affection has been attributed to other organs.

The liver, he obſerves, is often ſubjected to obſtructions, giving riſe to vomiting, where the cauſe of the affection is unknown. In proof of this aſſertion, he relates two caſes. In the firſt of theſe the patient, after being long ſubjected to violent vomiting, was cut off by hectic fever and atrophia. It was imagined, that the diſeaſe was ſeated in the ſtomach; but upon opening the body, that viſcus was found to be perfectly ſound, and it was diſcovered, that the affection entirely depended on an enlargement and obſtruction of the liver. The ſecond caſe is that of the Marchioneſs d'Epagny. In her ſimilar ſymptoms took place, and the vomiting was ſo ſevere as to render the cure almoſt deſperate. When M. Portal was called, by accurate examination, he diſcovered an enlargement of the liver. This led him to have recourſe to mercurials, and other aperients, by which a complete cure was effected. Since that time, he tells us, that he has treated ſeveral other patients in the ſame manner, with equal ſucceſs.

Another affection of the liver, which phyſicians often overlook, is violent hæmorrhage, happening from this viſcus, where the blood paſſes through the ductus communis into the inteſtinal canal, and from thence into the ſtomach. He relates ſeveral inſtances of this kind, and obſerves, that by diſſection after death, he has often detected miſtakes, the hæmorrhage, during the life of the patient, being ſuſpected to proceed from the ſtomach, and in ſome inſtances even from the lungs, while, in reality, it aroſe from the liver.

In concluding this memoir, M. Portal obſerves, that he has ſeen enlargements of the liver happily terminated by a large hæmorrhage. And as an example of this kind, he relates the caſe of Mr Aublet, a celebrated botaniſt, who was, in this manner, entirely freed from a tumour in the right hypochondrium, productive of very alarming ſymptoms.

From the different particulars mentioned in this memoir, M. Portal infers, that reſearches of this kind, founded on accurate obſervations, cannot fail to be of the higheſt utility, and that they well merit the attention of phyſicians.

IX.

Medical Reports, of the Effects of Tobacco, principally with Regard to its diuretic Quality in the Cure of Dropſies and Dyſuries: together with ſome Obſervations on the Uſe of Glyſters of Tobacco, in the Treatment of the Colic. By Thomas Fowler, *M. D. Phyſician to the General Infirmary of Stafford.* 8vo. London.

IN the treatiſe before us, a medicine is recommended, which has not, as far as we know, been hitherto uſed by other practitioners.

titioners. But if in their hands, the employment of this remedy be attended with the ſame good effects which have reſulted from its uſe, when directed by Dr Fowler, its introduction into practice in the manner here propoſed, may juſtly be conſidered as a diſcovery of very great utility. He ſets out with obſerving, that although the nature and cauſes of dropſies have been very fully inveſtigated, yet, that the indication of moſt importance, that of evacuating the water, has been attended with great difficulty.

Though diuretics are in many reſpects preferable to other evacuants, yet it unluckily happens, that no one has been diſcovered certain in its operation. A paragraph, he obſerves, in the third volume of theſe Commentaries, firſt induced him to adminiſter tobacco with this intention. It is part of a letter from Dr Garden at Charleſtown, to Dr Hope, wherein he ſays, "We here uſe with ſurpriſingly great efficacy in dropſical caſes, the alkaline fixed ſalt of tobacco." On reading this account, it occurred to our author, that if this ſalt be endowed with any medicinal virtue, ſuperior to that of common fixed vegetable alkali, it muſt ariſe from ſome property inherent in tobacco, and which perhaps had not been wholly deſtroyed by the action of fire. And if ſo, why not by the plant itſelf.

This circumſtance led our author to an experimental enquiry into the effects of tobacco, taken internally, and principally when employed under the form of infuſion. And the reſult of his enquiry has led him to conclude, that it may be exhibited in this manner, not only as a ſafe, but as an efficacious diuretic, in caſes of dropſies and dyſuries.

He begins by giving an account of its effects in the treatment of caſes of dropſy. The firſt caſe which he relates, is an inſtance of dropſical ſwelling of the legs, which was much relieved by taking an infuſion of tobacco. The infuſion here employed, was formed by macerating an ounce of the dried leaves of Virginian tobacco in a pound of boiling water, and adding to fourteen ounces of the liquor ſtrained off, after it had ſtood an hour, two ounces of rectified ſpirit of wine for preſervation. Of this infuſion the patient uſed at firſt only fifteen drops for a doſe, but it was gradually increaſed to two hundred drops twice a-day. It often produced ſickneſs at ſtomach, ſometimes vomiting, but was attended with the manifeſt effect of increaſing the quantity of his urine, and the almoſt total removal of his affection.

The ſecond caſe was ſtill more ſucceſsful; for, in this, dropſical ſwellings of the legs, of eight weeks continuance, were completely removed by the uſe of the infuſion of tobacco for ſeven days.

Beſides theſe, twenty other caſes giving an account of the effects of tobacco in dropſies, are related in this treatiſe. But we muſt refer thoſe, who wiſh for more particular information reſpecting theſe caſes, to the work itſelf, where they will find not only a hiſtory of the ſymptoms of the caſes, when the treatment was begun, but alſo regular reports, giving an account of the ſenſible effects of the medicine, and the progreſs of the cure. We may only conclude with obſerving, that in thirty-one dropſical caſes, in which he has employed it, eighteen were cured, and ten relieved. In the other three caſes, one a confirmed anaſarca in a woman of ſixty-one years of age, another a confirmed aſcites in a man of ſixty-three, and the third, a confirmed anaſarca, complicated with aſcites, in a man of ſixty-eight, it was employed without ſucceſs. Upon the whole, there can be no doubt that the ſucceſs of this infuſion as an hydropic medicine, has been uncommonly great. And if it be attended with equal benefit in the hands of others, its uſe in this manner may juſtly be conſidered as a moſt important diſcovery.

In the ſecond chapter, Dr Fowler treats of the effects of tobacco in caſes of dyſury. Here he preſents us with a particular account of ten caſes, in which he has uſed it. But he adds, that he has employed it alſo in eight others. Without entering into a particular account of any of theſe, we ſhall here preſent our readers with the general reſult of his experience on this ſubject. In four caſes of pure dyſury, in five, where this affection aroſe from gravel, and in one, where it proceeded from bloody urine, the uſe of the tobacco produced complete cures. In ſeven other caſes, two of pure dyſury, and one from gravel, it was productive of relief. And only one caſe of dyſuria, had, at the time of this publication, occurred to him in practice, where it was attended with no good effect.

To the account here given of the effects of tobacco in caſes of dropſy and dyſury, our author has added a third chapter, containing ſome obſervations on the uſe of tobacco-glyſters in the cure of colic. He tells us that he has directed glyſters of the infuſion of tobacco, in ſeveral obſtinate caſes of colic, reſiſting the ordinary means of cure, with conſiderable ſucceſs. They poſſeſs, he thinks, an anodyne and relaxant effect upon the ſyſtem in general, and a ſtimulating effect upon the rectum. By this means, they not only procure immediate eaſe, but a ſpeedy diſcharge of the indurated fæces. For an adult of an ordinary conſtitution, he conſiders an ounce of the infuſion as a medium doſe, when it is given under the form of glyſter. This he adviſes to be injected in half a pint of milk or common gruel. But if, in the ſpace of thirty or forty minutes, it procures no relief, excites no giddineſs or nauſea, and occaſions no diſcharge, he adviſes the repetition of the injection, with a larger quantity of the infuſion. By this mode of proceeding, the powers of the medicine, he obſerves, whether ſucceſsful or not, will, in a ſhort time, be fully tried; a matter of ſerious conſequence, where ſuſpenſe is diſtreſſing, and delay is dangerous.

Although Dr Fowler has chiefly employed an infuſion of tobacco, yet he thinks that other menſtrua alſo, as ſpirit, wine, or vinegar, may ſometimes, with advantage, be employed; and that, in ſome caſes, even it may be beſt exhibited in ſubſtance. To a pound of each of the different menſtrua mentioned, he adds an ounce of the leaves of Virginian tobacco; and after infuſion for a ſufficient length of time, for extracting the virtues of the plant, he thus forms an infuſum, tinctura, vinum, et acetum nicotianæ. For the preparation of the pills he propoſes the following formula:

℞. Pulveris foliorum nicotianæ Virginienſis cautè ſiccatorum.
Conſervæ roſarum rubrarum, utriuſque drachmam unam.
Mucilaginis gummi Arabici, quantum ſatis ſit. Miſce, fiat maſſa de qua pilulæ ſexaginta formentur.

It is unneceſſary to obſerve that in each pill one grain of the tobacco is contained. And while this is one of the moſt certain modes of exhibiting it, our author informs us, that, upon ſeveral trials, he has found theſe pills to be powerfully diuretic. The infuſion, however, with which he began, anſwered ſo well, that he has uſed it much more frequently than any other formula.

In the fifth and laſt chapter of this treatiſe, our author delivers ſome general obſervations on the effects of tobacco, with practical rules and cautions reſpecting its employment. He obſerves, that the immediate effect of the infuſion, in every caſe, is a pungent and tranſient ſenſation of heat in the throat, which is frequently ſucceeded by a ſenſe of warmth at the ſtomach, as if the patient had taken a dram. When taken in a moderate doſe, its next effect is that of proving diuretic, either with or without ſlight giddineſs. But, in a large doſe, it is more certainly diuretic, often laxative, generally attended with giddineſs, and frequently with nauſea. When pain exiſts, it has, for the moſt part, proved anodyne, in ſome caſes producing drowſineſs, and promoting ſleep. But, in others, the drowſineſs has been accompanied with a ſenſe of heat and reſtleſſneſs.

In particular caſes it has produced griping and purging, in others vomiting and ſweating, in others headach and trembling, and, in a few inſtances, contrary to its laxative quality, it has induced a tendency to coſtiveneſs. Dr Fowler has alſo met with two or three caſes where it cauſed a tranſient confuſion of ideas. But as theſe latter effects occur but ſeldom, and are of ſhort duration, they ſhould not, he thinks, be eſteemed a part of its ordinary operation.

From many experiments, Dr Fowler concludes, that the medium doſe of the infuſion of tobacco, for an adult, is about eighty drops twice a-day. And he thinks it moſt proper, to adminiſter it about two hours before dinner, and at bed-time. For, when given with an empty ſtomach, it is much more apt to excite ſickneſs, and other diſagreeable effects. He obſerves, however, that the proper doſe is much varied, not only by the age, ſex, and conſtitution, but alſo by the former habits of the patient. Thus he found, that twenty-five drops produced ſtronger effects upon a weak nervous woman, than four hundred on a man who was torpid through age, and accuſtomed to the uſe of tobacco. Taking, however, eighty drops as the medium doſe for an adult, he recommends ſixty for a patient of fifteen years of age, forty for one of ten, and twenty for one of five years. To children under five years, he has ſeldom ventured to adminiſter it.

When it has been continued for ſome weeks, it is, in general, neceſſary gradually to increaſe the doſe, in order to obtain the ſame degree of operation. In ſome caſes, it has been found advantageous even to intermit its uſe for a little, that the employment of it may again be reſumed with more advantage. And, upon repeating its uſe, after a ſhort interval, its former effects will again take place, although they had before that entirely ceaſed.

From the obſervations which Dr Fowler has made, relative to the medicinal properties of tobacco, he concludes, that it is very generally diuretic, thus proving an efficacious remedy in dropſical caſes: that it is anodyne, and frequently laxative, and thus uſeful in many painful caſes, where coſtiveneſs may render opiates exceptionable: that it is of great ſervice in caſes of dyſury, or painful difficulty in paſſing urine, by giving eaſe, and ſucceſsfully promoting the diſcharge: and that it is a moſt powerful remedy, by way of glyſter, in obſtinate caſes of colic.

He concludes his treatiſe with expreſſing his hope, that the candid reader will excuſe his errors and imperfections, in an attempt to explore the properties of a powerful, but neglected medicine. He obſerves, that more numerous facts are ſtill neceſſary, for aſcertaining, with preciſion, its influence on the human ſyſtem: that it ſtill affords a wide field for inveſtigation by experiment: and that, if his report ſhall induce others to make trial of its effects, and thus fully aſcertain it to be a valuable medicine, the intention of his publication will be anſwered. We cannot conclude our obſervations on this ſubject, without remarking, that, whether the trials of others ſhall or ſhall not, in every particular, coincide with thoſe made by Dr Fowler, he is ſtill entitled to much praiſe as a faithful and induſtrious obſerver.

X.

An Account of the Fox-glove, and ſome of its medicinal Uſes; with practical Remarks on Dropſy, and other Diſeaſes. By William Withering, *M. D. Phyſician to the General Hoſpital at Birmingham.* 8vo. Birmingham.

THE fox-glove, Dr Withering obſerves, the article of which he is here to treat, is a plant ſufficiently common in this iſland. But although it be in general well known, yet as the leaves of the common mullein, have ſometimes been gathered in place of it, he thinks it neceſſary to begin by giving a deſcription of this plant, and this deſcription is illuſtrated by a very elegant engraving. Without, however, entering into the particulars of this deſcription, it is ſufficient for us to obſerve, that it is the digitalis purpurea of Linnæus, a vegetable belonging to the ſecond order of his fourteenth claſs, the didynamia angioſpermia. But it is perhaps of more conſequence to remark, that it is referred by Linnæus to that natural order of plants which he ſtyles the *luridæ*, or, as they have been termed by others, the *ſolanaceæ*, and which comprehends moſt of the narcotic vegetables; and accordingly it has for its congenera, the nicotiana, atropa, hyoſcyamus, datura, ſolanum, and others of a ſimilar nature.

To the deſcription which our author gives of the digitalis, he ſubjoins a ſhort hiſtorical view of its properties, which was communicated to him by his valuable friend Dr Stokes of Stourbridge. From this view it appears, that ſince it was firſt mentioned by Fuſchius, in his Hiſtoria ſtirpium, it has been taken notice of, as employed for different purpoſes in medicine, by ſeveral ſucceeding writers, particularly by Dodoneus, Ray, Alſton, Haller, and Murray. But the accounts which they give of its ſenſible effects, as well as of its uſe, for medical purpoſes, are uncertain and unſatisfactory.

Our author's attention was firſt turned to the exhibition of the fox-glove, as a medicine, in the year 1775. And from obſerving its operation, in a variety of caſes, he found it to be a very powerful diuretic. The year following, it was gradually adopted by different medical practitioners, in the circle of our author's acquaintance.

ance. In the year 1779, a number of dropſical caſes occurred to our author in his practice, as the conſequence of a ſcarlet fever and ſore throat, which had raged very generally the preceding year. In all theſe caſes, the ſymptoms were very much alike; and they were without exception cured by the fox-glove. In the courſe of that year, Dr Stokes, then a ſtudent of medicine at Edinburgh, communicated to the Medical Society of that place, the reſult of Dr Withering's experience, in the uſe of this medicine. And, upon this, it was afterwards tried with ſucceſs in the Royal Infirmary at Edinburgh.

After having been for many years baniſhed from the Pharmacopœia of the Edinburgh College, it was again received as an article of the Materia Medica, in the edition of that work, publiſhed in 1783. Our author, however, expreſſes his ſuſpicion, that it will again be ſoon rejected from that liſt, if it ſhall be uſed in the unreſtrained manner, and in the enormous doſes in which it has of late been directed by ſome practitioners both in London and Edinburgh. In the courſe of ten years practice, experience and cautious attention have gradually taught our author how to uſe this medicine; and he here preſents the reader with a faithful account of the effects he has found to reſult from it, in a ſeries of caſes, commencing as early as the year 1775. Beſides the caſes treated by himſelf, he ſubjoins alſo ſome, communicated to him by correſpondents; in all amounting to about two hundred. From the nature of theſe caſes, we cannot here preſent our readers with any abridged view of them; but muſt refer thoſe who wiſh for particular information, to the treatiſe itſelf. We may, however, remark, that from this extenſive practice, he draws concluſions reſpecting the preparation and doſes of the foxglove, the ſenſible effects theſe produce, and the rules and cautions to be obſerved in the adminiſtration of it. He points out the differences in conſtitution, which may be conſidered as favourable or unfavourable to the ſucceſs of this medicine; and he concludes his treatiſe, with a few practical remarks on dropſy and ſome other diſeaſes, in which it may be ſucceſsfully employed. Of what he has advanced on theſe different particulars, we ſhall here endeavour to give a ſhort analyſis.

Though the root of the digitalis has not unfrequently been uſed for medical purpoſes, yet Dr Withering thinks, that the leaves are to be preferred to other parts of the plant. Theſe, he adviſes, to be gathered after the flowering ſtem has ſhot up, and about the time when the bloſſoms are coming forth. He directs, that the leaf-ſtalk and mid-rib of the leaves ſhould be rejected, and that the remaining part ſhould be dried either in ſun-ſhine or before the fire. If the leaves, in this condition, be well dried, they rub down to a beautiful green powder. And in this ſtate, they may be employed for medical purpoſes. From one to three grains of this powder may be given for a doſe twice a-day. It may be exhibited either by itſelf, or conjoined with ſome aromatic, or it may be formed into pills with ſoap or gum ammoniac. For exhibiting the digitalis in a liquid form, he directs, that a dram of the dried leaves be infuſed for four hours in half a pint of boiling water, and that there be added to the ſtrained liquor, an ounce of any ſpirituous water, for its preſervation. Of this infuſion, he conſiders one ounce as a medium doſe for an adult. This may be given twice a-day, or with ſome it may be given once in eight hours; on the contrary, with others half an ounce will be a ſufficient quantity at a time. When the digitalis is diſpoſed to excite looſeneſs, which ſometimes happens, opium may be advantageouſly joined with it; and, on the other hand, when the bowels are tardy, jalap may be given at the ſame time with the digitalis, without interfering with its diuretic effects.

When the fox-glove is given in large doſes, quickly repeated, it occaſions, ſays our author, ſickneſs, vomiting, purging, giddineſs, confuſed viſion objects appearing yellow or green, increaſed ſecretion of urine, with frequent motions to part with it, and ſometimes inability to retain it, ſlow pulſe, ſo as even not to beat above thirty-five ſtrokes in a minute, cold ſweats, convulſions, ſyncope and death; and he expreſſes a doubt, whether it does not, in ſome inſtances, excite a copious flow of ſaliva. When it is given in ſmaller doſes, he has found it produce moſt of theſe effects in a ſmaller degree. He points it out as a curious circumſtance, that, with a certain doſe of this medicine, the ſickneſs does not take place till many hours after its exhibition has been diſcontinued.

The flow of urine, he obſerves, will ſometimes precede, ſometimes accompany, ſometimes follow the ſickneſs, at the diſtance of ſeveral days, and not unfrequently be checked by it. The ſickneſs thus excited, is, he obſerves, different from that occaſioned by other medicines. After ceaſing, it will recur again as violently as before, and will continue by repeated attacks, in this manner, for three or four days, at diſtant and more diſtant intervals.

Dr Withering firſt imagined, that, in order to inſure the diuretic effects of the digitalis, it was neceſſary to bring on and continue the ſickneſs. But from attentive obſervation, he ſoon found, that the diuretic effects would often take place firſt, and ſometimes be checked, when the ſickneſs or purging ſupervened. And from extenſive experience, he diſcovered, that the good effects of this medicine might be moſt certainly obtained, by enjoining his patients to obſerve the following directions with regard to its uſe. Let the medicine be given to adults in doſes of an ounce of the infuſion, or three grains of the powder twice a-day. Let it be continued till it either acts upon the kidneys, the ſtomach, the pulſe, or the bowels; but let it be ſtopped on the firſt appearance of any of theſe effects. By exhibiting it in this manner, he contends, that the patient will neither ſuffer from its exhibition, nor the practitioner be diſappointed in any reaſonable expectation.

During its operation, he remarks, that patients ſhould be enjoined to drink very freely. In aſcites, and even anaſarca, when the patients are weak, and the evacuation of the water rapid, the uſe of proper bandages becomes indiſpenſably neceſſary for their ſafety. If the water be not wholly evacuated, in conſequence of a firſt exhibition, in the manner mentioned above, an interval of ſeveral days ſhould be allowed, that proper food and tonics may be adminiſtred.

Beſides the mode of exhibition, which has already been mentioned, our author, from ſome late trials, is inclined to believe, that the digitalis, in doſes of two or three grains a-day, may remove dropſies, without producing any other effect than that of a mild diuretic: and that, in this manner, it may be given without any interuption of its uſe, till the cure be completed.

When diſtreſſing ſickneſs ariſes from the incautious uſe of the fox-glove, the knowledge of a remedy to counteract this, and other diſagreeable effects, would be of very great uſe. In ſuch caſes, the uſual cordials and volatiles are generally rejected from the ſtomach, aromatics and ſtrong bitters are longer retained, and when the ſickneſs was only ſlight, it has ſometimes been removed by brandy. In one caſe, an infuſion of mint was attended with a good effect. He has ſometimes thought, that opium, in ſmall doſes, was of advantage. And he is very confident, that benefit has been derived from the application of bliſters.

With reſpect to the conſtitution of patients, he obſerves, that there are ſome favourable, others unfavourable, to the ſucceſs of the digitalis. It ſeldom, he obſerves, ſucceeds in men of great natural ſtrength, of tenſe fibre, of warm ſkin, of florid complexion, or in theſe with a tight and cordy pulſe. He has but little hope from it in theſe caſes of aſcites, where the belly is tenſe, hard, and circumſcribed; and in anaſarca, where the limbs feel ſolid, and give much reſiſtance to preſſure. On the contrary, we may, he tells us, expect the diuretic effects to follow in a kindly manner, if the pulſe be feeble or intermitting, the countenance pale, the lips livid, the ſkin cold, the belly ſoft and fluctuating, or the limbs readily pitting upon preſſure.

From the extenſive practice which Dr Withering has had in the uſe of the digitalis, he deduces the following inferences, which he apprehends are ſupported by facts, that the digitalis will not univerſally act as a diuretic, but that it more generally produces this effect than any other medicine, and often after theſe have been tried in vain: that if this fails there is but little chance of any other medicine ſucceeding: that in proper doſes, and under the management now pointed out, it is mild in its operation, and gives leſs diſturbance to the ſyſtem than ſquills or any other active medicine: that when dropſy is attended by palſy, unſound viſcera, great debility, or other complication of diſeaſes, neither the digitalis, nor any other diuretic, can do more than obtain a truce to the urgent ſymptoms; but that it may be uſed with advantage in every ſpecies of dropſy, excepting the encyſted; that it may be ſubſervient to the cure of diſeaſes unconnected with dropſy;

and finally, that it has a power over the motion of the heart, to a degree yet unobſerved in any other medicine, in conſequence of which, it may probably be employed in accompliſhing ſalutary ends.

This treatiſe, as we have already obſerved, is concluded with ſome practical remarks on dropſy and ſome other diſeaſes.

Anaſarca, he obſerves, is generally curable when ſeated in the ſubcutaneous cellular membrane, or in the ſubſtance of the lungs. Since anaſarcous ſwellings often take place in palſied limbs, in arms as well as legs, there is reaſon, he thinks, to infer, that the ſwelling does not depend merely upon poſition; and he ſuſpects, that many dropſies originate from the paralytic affections of the lymphatic abſorbents. Suppoſing this, however, to be the caſe, is it not, he obſerves, probable that digitalis, which is ſo effectual in removing dropſy, may alſo be advantageouſly uſed in ſome kinds of palſy?

When epilepſy depends upon effuſion, Dr Withering thinks the digitalis will effect a cure; but he adds, that in other kinds of epilepſy it has not had a ſufficient trial in his hands, to enable him to determine what it can do.

Dr Withering is inclined to believe with Dr Quin and others, that hydrocephalus is a diſeaſe which originates in inflammation, and that the water found in the ventricles of the brain after death, is the conſequence, not the cauſe of the illneſs. And he thinks, that, at the commencement, the cure is to be attempted by repeated topical bleedings, vomits, and purges. He has not yet been able to determine, from experience, whether the digitalis can with advantage be uſed in the ſecond ſtage of the diſeaſe. But when we conſider the enormous quantities of mercury which may be uſed in this complaint, without affecting the ſalivary glands, he thinks it probable, that other parts may be equally inſenſible to the action of their peculiar ſtimuli; and, therefore, that digitalis ought to be given in much larger doſes, in this than in other diſeaſes. He apprehends inſanity to be more frequently connected with ſerous effuſion, than has been commonly imagined. And where appearances of anaſarca point out this cauſe of the complaint, the happieſt effects may, he thinks, be expected from the digitalis.

In caſes of phthiſis pulmonalis, the digitalis has been much uſed by the common people in the weſt of England; and in a manuſcript note, written, he believes, by Mr Saunders, who was many years ago a practitioner of great eminence at Stourbridge, conſumptions are repreſented as infallibly cured by a weak decoction of fox-glove leaves in water, or wine and water, taken for common drink. In this diſeaſe, however, he acknowledges, that in his own hands it has done but little ſervice. He expreſſes a wiſh, however, that others would undertake the enquiry.

From the account now given of the digitalis, we are naturally led to conclude, that it is a medicine not only highly efficacious, but extenſively uſeful. Whether future obſervations will confirm or refute thoſe of Dr Withering, we cannot pretend to foretel. And we may conclude in his own words, that in ſpite of opinion, prejudice, or error, time will fix the real value upon this diſcovery, and determine whether he has impoſed upon himſelf and others, or contributed to the benefit of ſcience and mankind. But whatever the deciſion may be, we apprehend that every candid reader who beſtows an attentive peruſal on this treatiſe, will allow that the author is not only juſtly entitled to a high degree of praiſe for many ingenious obſervations, but that he cannot be too much applauded for the attention he has beſtowed in the collection and preſervation of uſeful facts, and in freely communicating theſe to the public for the information of others.

XI.

Everardi Joannis Thomaſſen A. Thueſſink, *A. L. M. Philoſ. Doct. Soc. Reg. Med. Edn. et Hiſt. Natur. Stud. Edin. Sodal. nec non Muſæi Pariſini, ac Soc. Phyſico-Med. Hagan. a Comm. Literario, Diſſertatio de Opii Uſu in Siphylide, Obſervatis probato.* 8vo. Ludg. Bat. 1785.

IN a ſhort introduction prefixed to this diſſertation, Dr Thueſſink obſerves, that among all the vegetables which the earth produces, the juice of the poppy, particularly that which is brought from India, under the form of an inſpiſſated gummy reſinous ſubſtance, and which is known by the name of *opium*, has long held the firſt place as a medicine. To this the phyſician has recourſe as a certain means, if not of removing, at leaſt of alleviating the moſt excruciating pains. Yet in all ages it has been employed, not only with ſome degree of caution, but even of terror. Hence, he thinks, we are to account for the diſcovery of its effects, in particular diſeaſes, being but of late date. And it is only, he obſerves, but a very few years ſince it was firſt employed as a remedy in venereal complaints. In this diſeaſe recourſe has lately been had to it in Britain, not only as a means of alleviating thoſe nocturnal pains in the bones, to which ſiphylitic patients are often ſubjected, but for healing ulcers ariſing from the ſame cauſe: and in the progreſs of its uſe, it has been diſcovered, that it is not only an excellent auxiliary in different caſes, but that even by itſelf it has been able to accompliſh complete cures, where mercury had been tried in vain, and the patient was left in the moſt deplorable ſituation.

As the diſcovery of many of the moſt valuable remedies has been the effect of accident, ſo our author obſerves, this owed its introduction to a young man, who, for the ſpace of ſeveral years, had in vain tried mercury under a variety of forms, with the view of combating an obſtinate ſiphylis, to which he was ſubjected. In this ſituation he had recourſe to large doſes of opium for mitigating a diſeaſe which he no longer hoped to be able to eradicate. But by the liberal uſe of this article, he was, in no long time, in a manner that exceeded all expectation, reſtored to perfect health.

This wonderful fact being communicated to Dr T. Michaelis, phyſician-general to the Heſſian troops in America, he reſolved, by accurate experiments, to determine the extent of this power. Accordingly, in many of the American hoſpitals, but particularly in that of New York, trials were made, and in general with ſucceſs. Information of this being brought to Europe, opium was immediately employed as a cure for ſiphylis in different hoſpitals in Britain, particularly by Dr William Saunders in Guy's Hoſpital in London, and by Dr Henry Cullen in the Royal Infirmary of Edinburgh.

In the treatiſe before us, Dr Thueſſink propoſes to preſent his readers with a candid account of facts on this ſubject, as far as he has been able to collect them. And with this view, he propoſes, in the firſt place, to give an account

count of the ſenſible effects of opium, in a man not labouring under the venereal diſeaſe, and to compare theſe with its effects on a venereal patient, from thence deducing the regimen and method of employing it in venereal affections. In the next place, to exhibit a view of its influence in combating and mitigating that affection. And finally, to draw from the whole ſome uſeful concluſions.

Following the plan laid down, he treats in the firſt chapter of the effects of opium, and the method of exhibiting it. He ſets out with obſerving, that a controverſy has been very keenly agitated among medical practitioners, Whether opium is to be conſidered as a ſtimulant or ſedative medicine. Even thoſe, however, adopting the former opinion, although they contend that immediately after opium is taken, it increaſes the action of the muſcular fibre, yet admit that a ſtate of collapſe or debility follows this, which they term *indirect*. Hence he juſtly obſerves, that, in reality, the only difference is in the one, ſuppoſing it to be an indirect, the other a direct ſedative. And indeed, when it is in daily uſe, not for exciting, but for allaying action; not for arouſing, but for inducing ſleep; not for occaſioning, but for mitigating pain, it is abſurb to term it a ſtimulant medicine. And with reſpect to the manner in which it acts as a ſedative, he adds, that from the moſt accurate experiments which he was capable of making, he never was able to diſcover what could be called a ſtimulant power during its operation; for it had uniformly the effect, from the beginning, of diminiſhing the celerity of the pulſe, and of rendering reſpiration ſlower. In ſome caſes nauſea and paleneſs of the countenance took place, attended with a ſenſe of ſleepineſs, and a diminution of all ſecretions and excretions, ſometimes even of perſpiration. If he took the opium to the quantity of two grains or upwards, ſleep immediately enſued, which he was incapable of reſiſting, but which was ſoon diſturbed by dreams; and when he awaked from it, he was not refreſhed, but ſubjected rather to nauſea.

When, however, the effects of opium given to ſiphylitic patients are compared with thoſe which take place when it is given to people in health, or even ſubjected to other diſeaſes, they are, he alleges, very conſiderably different. Siphylitic patients bear, he obſerves, a much greater quantity, without any inconvenience.

In America three grains were at firſt given in the courſe of a day; and this was ſoon gradually increaſed to the quantity of a ſcruple, or even half a dram. In this diſeaſe it rarely affects either the pulſe or reſpiration; but when it does, it renders them ſlower. It never affects the appetite, unleſs in thoſe caſes where it has been diminiſhed by pain, when it often reſtores it to the natural ſtate. It very ſeldom produces here either nauſea or vomiting; and although in health, in a ſhort time, it induces obſtinate conſtipation; yet in this diſeaſe, it often gives riſe to diarrhœa. Its effects, on various ſecretions alſo, are here wonderfully altered. With one it is found to increaſe, with another to ſuppreſs perſpiration, and to produce a remarkable diureſis. And in ſome caſes, as well as mercury, it gives riſe to ſalivation.

With many venereal patients, when taken even to the quantity of a ſcruple in the day, it has produced no change on the ſtate of ſleep, the patient riſing and going to bed at his uſual hours. In the greater part, however, of thoſe caſes which our author has himſelf had an opportunity of obſerving, it has given riſe to a certain degree of ſopor and indolence. In ſome caſes it has produced a ſhaking of the hands; but this ſymptom ſoon ſpontaneouſly ceaſes; which is alſo the caſe with headach, a ſymptom, however, rarely occurring from the uſe of opium in this affection. But of all the ſingularities attending the uſe of opium in this diſeaſe, what he conſiders as one of the moſt remarkable, is, that no habit is contracted, rendering it afterwards neceſſary to continue its uſe, an event very generally taking place from its employment both in health and in other diſeaſes.

As an antiſiphylitic, opium may be exhibited either in ſubſtance or under the form of Thebaic tincture. It is always prudent to begin its uſe with ſmall doſes, as a grain at bed-time, and gradually to increaſe it by a grain in the day, till it be taken daily to the quantity of a ſcruple, half a dram, or even more, divided into different doſes. But as ſoon as it produces a favourable change on the ſymptoms, which is often the conſequence of its' being taken to the quantity of ſix or eight grains in the day, it ſhould not be puſhed farther.

The regimen during the uſe of opium in this diſeaſe muſt be entirely accommodated to the condition of the patient. In general, however, no great ſtrictneſs in this particular is required, and moderate exerciſe is always to be enjoined.

After theſe obſervations on the effects of opium, and on the method of exhibiting it, our author proceeds, in the ſecond chapter, to treat of its uſe in different venereal affections. Although opium would naturally be concluded to be highly prejudicial in gonorrhœa, by thoſe who hold it to be a ſtimulant medicine, yet Dr Thueſſink obſerves, that, in reality, the beſt effects are derived from the uſe of it, both when taken internally, and when uſed under the form of injection. In ſupport of the former practice, he mentions the authority of that juſtly celebrated practitioner Mr John Hunter of London, who ſtrongly recommends liquid laudanum, to the extent even of forty drops every four hours, againſt the moſt diſtreſſing ſymptoms of this diſeaſe, particularly the painful erections, ardor urinæ, chordee, and even eryſipelatous inflammation tending to gangrene. He gives the following formula for the employment of opium under the form of injection: ℞. Opii ʒii, aq. font. ℥xii ſolve, dein adde ſolut. cuilibet ℥ſs. Sach. ſaturn. gr. viii. pro inject. This injection, he informs his readers, is employed by that excellent practitioner Dr James Hamilton, in the Royal Infirmary of Edinburgh, in every ſtage of gonorrhœa, even the earlieſt, and with the greateſt imaginable ſucceſs. Beſides caſes which have occurred to himſelf, in confirmation of theſe authorities, he concludes his ſection on this ſubject, by relating a caſe communicated to him by his ingenious friend Dr Lyons of Philadelphia, in which an obſtinate and ſevere gonorrhœa was cured by opium alone.

In caſes of phymoſis and paraphymoſis, he affirms, that nothing is ſo efficacious as large doſes of opium; and he ſupports this practice, not only by his own experience, but alſo by the authorities of Drs Simmons, Schwediauer, Cullen, and Michaelis. He ſtrongly recommends opium alſo in venereal iſchuria, and mentions a caſe which occurred in his own practice, where opium, taken internally, and uſed under the form of glyſter, was productive of immediate relief.

As ſwelling of the teſticle in venereal caſes often ariſes from an affection of the urethra, he thinks that a plaſter of pure opium, applied externally to the urethra, may be productive of good effects; but he does not recommend this practice on any experience he has yet had of its efficacy. Although he is not of opinion that opium is the beſt remedy for diſcuſſing venereal buboes, yet he informs us, that, in one caſe, Dr Michaelis employed it, from three to ſix grains in the day, with the effect not only of diſcuſſing the bubo, but alſo of healing venereal ulcers, which at the ſame time took place. But he beſtows ſtill higher praiſes on the uſe of opium in thoſe caſes where buboes have terminated in ſuppuration. And its efficacy in ſuch inſtances, he ſupports by caſes in which it was employed with the beſt effect, by different practitioners, particularly by Dr Michaelis and Mr Grant.

What have hitherto been mentioned, he conſiders as inſtances where ſiphylis is merely topical. But in the laſt ſection of this chapter, he treats of the uſe of opium in thoſe caſes where venereal ulcers, and other ſymptoms, ariſe from the abſorption of the venereal virus into the circulating ſyſtem. On this ſubject, he has collected upwards of thirty caſes, ſome treated by Dr Michaelis, others by Mr Grant, and others by Dr H. Cullen, in the Royal Infirmary at Edinburgh, which he had himſelf an opportunity of obſerving. Without pretending to mention the particulars of theſe caſes, we may only obſerve, that, in a variety of venereal affections, the uſe of opium was attended with the happieſt effects. And, in confirmation of this practice, he mentions alſo the authority and experience of Dr Nooth, phyſician to the Britiſh army in America, of Dr Saunders of London, of Dr Webſter of Edinburgh, and ſeveral other practitioners. But, at the ſame time, he candidly acknowledges, that it has not been equally ſucceſsful with every practitioner who has had recourſe to it; and he mentions particularly Mr Pearſon of the Lock Hoſpital, and Dr Withering of Birmingham, the latter of whom conſiders opium, in ſiphylis, as being only a fallacious and palliative remedy.

After relating theſe facts concerning the influence of opium in different venereal complaints, Dr Thueſſink concludes this treatiſe by offering, in his third chapter, ſome animadverſions on this ſubject. He thinks that its operation may be referred to three heads: that it acts as a remedy, mitigating and alleviating the ſymptoms; that it acts as a primary remedy, but by its ſedative power alone; and that it acts as a primary and antiſiphylitic remedy, deſtroying the venereal virus. In the firſt of theſe ways, he conſiders it as operating in gonorrhœa, phymoſis, paraphymoſis, venereal iſchuria, ſwelling of the teſticle, and ſymptomatic bubo. In the ſecond way, he conſiders it as operating in thoſe caſes where obſtinate venereal ulcers have remained, after the long continued uſe of mercury. And we have, he thinks, inconteſtable proofs of its effects in the third way, in thoſe caſes where ſiphylis has been cured by the uſe of opium, although no mercury had been taken.

Dr Thueſſink, however, obſerves, that many apparently ſtrong objections had been urged to the uſe of opium in this diſeaſe. It has been alleged, that the cures which follow the uſe of opium do not prove its antiſiphylitic power: that there are many caſes in which opium has not ſucceeded: that this remedy, when uſed to the extent here required, often produces effects worſe than the diſeaſe itſelf: that it is only a palliative remedy, concealing, as it were, for a time, the ſymptoms of the diſeaſe: and, finally, that, even ſuppoſing it to poſſeſs the alleged antiſiphylitic power, this is but of little importance, ſince, in mercury, we already poſſeſs a ſafe and efficacious remedy in this diſeaſe. To theſe different objections, our author urges, very ſenſible, and what may, in general, be conſidered as very ſatisfactory anſwers. And he thinks it may juſtly be concluded, that opium, in various caſes, is a moſt uſeful auxiliary medicine, not only in alleviating many venereal ſymptoms, but in tending to the cure of ulcers ariſing from ſiphylis, which, however, continue after the virus has been overcome; and that, in theſe caſes, where mercury is either contraindicated, or where that ſpecific has failed, opium, in large doſes, may be employed, both with ſafety and advantage.

While we moſt heartily applaud the induſtry which Dr Thueſſink has employed in the collection of facts on this important ſubject, we cannot help expreſſing our earneſt wiſh that more extenſive and longer-continued experience may afford inconteſtable evidence of the antiſiphylitic power of opium, which, notwithſtanding the efficacy of mercury may juſtly be conſidered as a very important diſcovery in the practice of medicine. But, notwithſtanding the facts which Dr Thueſſink has collected, we muſt own, that we are not altogether without doubts on this ſubject. Among other particulars, the alleged power of opium of inducing ſalivation, in venereal caſes, rather creates ſuſpicion than affords confirmation of this doctrine. No caſe has occurred, in our practice, nor do we recollect to have read or heard of any caſe where opium, employed even to the greateſt extent, in other diſeaſes, was found to induce ſalivation. When this diſcharge, therefore, takes place, with venereal patients, under the uſe of opium, is there not reaſon to ſuſpect that the patient may have taken mercury without the knowledge of the phyſician? And, if there has been deception in theſe caſes, may it not have taken place in others where no ſalivation occurred? When ſalivation is alleged to occur from opium, we could wiſh that particular attention were paid to the taſte in the mouth, the ſmell of the breath, the appearance of the gums, and other particulars, which are the common concomitants of a mercurial ſalivation, as a due regard to theſe might tend to detect impoſture. We readily, however, admit, that opium may have an antiſiphylitic power, although it have no influence in inducing ſalivation. And we think it is now inconteſtably proved, that, even ſuppoſing it to poſſeſs no antivenereal power, it is a remedy much more extenſively uſeful in venereal complaints than was formerly imagined. But whether the good effects derived from it are entirely to be attributed to its ſedative power, to which the ingenious author of this treatiſe refers much of its operation, or are to be explained in any other manner, muſt be determined by future experience.

XII.

Deſcription of a Plant yielding Aſafœtida, in a Letter from John Hope, *M. D. F. R. S. to* Sir Joſeph Banks, *Bart. P. R. S. Vid. Philoſophical Tranſactions.* Vol. LXXV. Part 1. 4to. London.

IT is near a thouſand years, Dr Hope obſerves, ſince aſafœtida was firſt uſed in medicine, but it is not above ſeventy years ſince we had any ſatisfactory account of the plant which yields it. Kaempfer, who, towards the end of the laſt century, travelled over a great part of Aſia, and was in thoſe parts of Perſia where the aſafœtida is collected, not only gives a full account, in his Amoenitates Exoticæ, of the method of obtaining it, but alſo deſcribes the plant from whence it is obtained, but it had never been cultivated in any European garden, till Dr Pallas was fortunate enough to raiſe it from ſome ſeeds which were ſent from the mountains of Ghilanin in Perſia.

In the year 1777, Dr Guthrie of St Peterſburgh, gave to Dr Hope two roots of it which he had obtained from Profeſſor Pallas. Both theſe roots were planted in the open ground in the botanic garden at Edinburgh. One of theſe died, but the other did well. It has lately flowered and produced ſeed.

Dr Hope availing himſelf of this opportunity, preſented the Royal Society of London with a minute and accurate deſcription of this plant, which is now publiſhed in their tranſactions, and illuſtrated by two elegant engravings, the one exhibiting the flower, the other the leaves of the plant. As Dr Hope's deſcription will not admit of any abridgment, we muſt here content ourſelves with obſerving, that the aſafœtida growing in the botanical garden at Edinburgh, is an umbelliferous plant, of a pale ſea-green colour, which grows to the height of about three feet. That the ſtem is deciduous, but the root perennial. That every part of the plant, when wounded, pours forth a rich milky juice, reſembling aſafœtida both in ſmell and taſte. At times alſo, a ſmell, ſuch as a faint impregnation of aſafœtida yields, was perceivable at the diſtance of ſeveral feet.

Although, however, from all theſe circumſtances, there is reaſon to conclude, that this is a plant which produces true aſafœtida, yet Dr Hope has obſerved, that the deſcription and drawing preſented by him to the Royal Society, differ in many reſpects from thoſe formerly publiſhed by Kaempfer. Sir Joſeph Banks ſuggeſts, in a note annexed to this paper, that gummi-reſinous ſubſtance, known by the name of aſafœtida, may, as well as the ſanguis draconis, and ſome other gums, be indifferently the produce of various ſpecies of plants; and that while Kaempfer has deſcribed one, Dr Hope has deſcribed another.

With regard to the method of obtaining gum aſafœtida, Dr Hope obſerves, on the authority of Kaempfer, that, at the proper ſeaſon, the root is cut over once and again: that from the inciſion there flows a thick juice, like cream; and that this, farther thickened, is the aſafœtida of the ſhops. As the juice which the plant growing in the botanical garden at Edinburgh yields, ſeems evidently to be the ſame with the officinal aſafœtida; and as the plant bears the viciſſitudes of our climate, growing in the open air without protection, and, even in an unfavourable ſeaſon, producing a good deal of ſeed, he expreſſes his hope, that it may become an article of cultivation in this country, of no inconſiderable importance.

In addition to the above extract from the Tranſactions of the Royal Society, we may obſerve, that the aſafœtida plant, in the botanic garden of Edinburgh, has now flowered three different times, *viz.* in the year 1782, in 1784, and in 1785. But we are ſorry to learn, that none of its ſeeds, though put into the hands of very able botaniſts and gardeners, have vegetated.

With the deſcription of the plant growing at Edinburgh, Dr Hope ſent to the Royal Society ſeveral other accurate drawings, particularly one exhibiting a full view of the entire plant in flower, which being too large for the ſize of the Philoſophical Tranſactions, was not engraven. We are glad, however, to hear, that all theſe drawings were alſo ſent by Dr Hope's direction to Dr Pallas, who wiſhed to publiſh, in the Flora Roſſica, a deſcription and delineation of this rare plant. It is alſo probable, that, to gratify the curioſity of this country, Dr Hope will publiſh an engraving of the entire plant, in its moſt perfect ſtate.

XIII.

The Structure and Phyſiology of Fiſhes explained, and compared with thoſe of Man, and other Animals; illuſtrated with Figures. By Alexander Monro, *M. D. Fellow of the Royal College of Phyſicians, and of the Royal Society, and Profeſſor of Phyſic, Anatomy, and Surgery, in the Univerſity of Edinburgh.* Folio. Edinburgh.

IN the introduction to this treatiſe, Dr Monro obſerves, that a variety of curious circumſtances had occurred to him, in examining the ſtructure of fiſhes, ſome of which had been entirely overlooked, others imperfectly deſcribed by authors. And as theſe relate to points of chief importance in the animal œconomy, he flattered himſelf that an account of them would not be leſs acceptable to the phyſician than to the naturaliſt.

Including under the claſs of fiſhes, both the nantes pinnati, and piſces of Linnæus, he employs the raja, or ſkate, as his chief example of the former, and the gadus, or cod, of the latter. But he occaſionally deſcribes alſo parts of other fiſhes; and his deſcriptions are illuſtrated by figures, repreſenting the parts of their natural ſize, contained in fifty tables, which we have no doubt are delineated with the moſt faithful accuracy.

From the nature of this work, the reader will neceſſarily conclude, that any account which could here be given of it muſt be very imperfect: for a proper idea of the author's meaning can be had only from a careful examination of the plates themſelves. Here, therefore, we ſhall only endeavour to give ſome view of the contents of this work, and of the phyſiological remarks which our author has deduced from his deſcriptions. He begins by treating of the circulation of the blood in fiſhes: he next makes ſome remarks on their organs of ſecretion: he then gives an account of their abſorbent ſyſtem: and he concludes with ſome obſervations on their brain, nerves, and organs of ſenſe. But, that the reader may be enabled to form a more exact idea, not only of our author's plan, but of the facts he deſcribes, on which he founds his opinion, we ſhall here annex a table of the contents, both of the text, and likewiſe of the figures by which his deſcriptions are illuſtrated.

Chap. I. A deſcription of the circulation of the blood in fiſhes.

II. Obſervations on the circulation of the blood in fiſhes.

III. Of the glandular and ſecreted fluids in fiſhes.

  Sect. 1. Of the liquors ſecreted on the external ſurface of fiſhes.

  2. Of the liquors ſecreted into the cavities of the cranium, pericardium, and abdomen.

  3. Of the liquors ſecreted into the organs of digeſtion.

  4. Of the ſecretions of the male organs.

  5. Of the ſecretions of the female organs; of the nutrition of the fœtus.

  6. Of the ſwimming bladder of fiſhes.

IV. A deſcription of the ſyſtem of abſorbent lymphatic veſſels in fiſhes.

  Sect. 1. A deſcription of the ſyſtem of abſorbent lymphatic veſſels in the car-

tilaginous fiſhes, or nantes pinnati of Linnæus.
  2. A deſcription of the ſyſtem of abſorbent lymphatic veſſels in the oſſeous fiſhes, or piſces of Linnæus.

V. Experiments and obſervations on the ſyſtem of abſorbent lymphatic veſſels of fiſhes.

VI. Obſervations on the lymphatics of the ſpleen in fiſhes, and on the uſes in general of that organ.

VII. Of the firſt diſcovery of the ſyſtem of abſorbent, lacteal, and lymphatic veſſels, in birds, fiſhes, and amphibious animals.

VIII. Of the brain and organs of ſenſe in fiſhes.
  Sect. 1. Of the brain in fiſhes, and nervous ſyſtem in general.
  2. Of the noſe in fiſhes.
  3. Of the ear in cetaceous fiſhes.
  4. Of the ear of amphibious animals, and particularly of the ſea tortoiſe or turtle.
  5. Of the ear in the cartilaginous and oſſeous fiſhes.
  6. Of the ear of a ſkate.
  7. Of the ear of the ſqualus ſquatina, or angel fiſh.

8. Of the ear in ſome of the long ſhaped cartilaginous fiſhes, and in the oſſeous fiſhes.

IX. Experiments on hearing in water.

X. Of the ſeveral ways in which the tremor of ſonorous bodies is communicated, in different claſſes of animals, to the nerves ſpread on the bottom of the ear.

XI. Of the eyes in fiſhes.

XII. The anatomy of the cuttle fiſh, or ſepia loligo.

XIII. The anatomy of the ſea egg, or echinus marinus.

Tab. 1. Repreſents the extent of the ſurface of the gills, the heart, the branchial arteries and veins, and the aorta of a ſkate, filled with injection.

2. The thoracic and abdominal viſcera, with the circulating veins, of a female ſkate.

3. The arteries and circulating veins of the chylopoetic viſcera of a ſkate, with a ſlight ſketch of the lacteals, all filled with injection.

3*. The muſcular coat of the meſenteric vein of an ox.

4. The heart and large veſſels of a ſea tortoiſe, or turtle.

5. A large lymphatic veſſel ſituated on the ſide of the cod fiſh, and likewiſe the principal ducts which pour out mucus on the ſurface of the body.

6. The mucous ducts on the fore part of a ſkate.

7. The ducts which pour out mucus on the back part of a ſkate, and likewiſe the meatus auditorii externi.

8. Two ſmall canals at the ſides of the anus in the ſturgeon, which lead into the cavity of the abdomen; and two other paſſages by which the cavity of the abdomen in that fiſh communicates with the pelvis of the kidneys.

9. Principal parts of the ſtructure of the abdominal viſcera in a ſmall female ſkate.

9*. The ſtructure of the pancreas, and of the villous coat of the inteſtines in the ſturgeon.

10. The biliary ducts in a cod, with the termination of the inteſtinula cœca.

10*. The liver, gall-bladder, and biliary ducts, of the wolf, or cat fiſh.

11. The organs of generation, and of urine, in the male ſkate.

12. The organs of urine, and part of the organs of generation in the male ſkate.

13. The termination of the uteri and veſica urinaria of a ſkate, in the common cloaca.

14. The fœtus of a ſkate cut open; the duct which connects the yolk of the egg to the inteſtinal tube.

14*. A young fœtus of a ſkate, with its blood-veſſels, before the gills are covered.

15. A duct by which the ſwimming bladder in the ſalmon, the carp, and the herring, communicates with the œſophagus or ſtomach; and a vaſcular ſubſtance in the air-bag of a cod.

15*. The heart, ſtomach, air-bag, and air-duct, of the conger, or ſea eel.

16. A large communication between the ſwimming bladder of the ſturgeon and his ſtomach.

17. The abdomen of a female frog laid open.

18. The abdominal viſcera of a ſkate, with the lacteal and lymphatic veſſels filled with wax, and their termination on the right ſide of the body.

19. The heart and abdominal viſcera of a ſkate, and the lacteal and lymphatic veſſels

filled with wax, with their termination on the left ſide.

20. A vaſcular and cellular receptacle of chyle, ſituated on the ſtomach of a ſkate.

21. The lymphatic veſſels of the brain, the ear, the eye, and the ſkin of a ſkate injected with wax.

22. The heart and abdominal viſcera of a cod-fiſh, with the lacteals, receptacle of the chyle, and lymph injected with wax.

23. Portions of the lacteals of a cod, injected with wax of their natural ſize, and alſo as viewed with a magnifying glaſs.

24. The joining of the lacteal with the lymphatic veſſels in the cod.

25. The receptacle of the chyle and lymph in a haddock.

26. The branchial artery, venæ cavæ, and terminations of the lymphatic ſyſtem in a ſalmon.

27. The joining together of the principal lymphatic veſſels and terminations of the lymphatic ſyſtem in a ſalmon.

28. The terminations of the lymphatic ſyſtem, and hepato-cyſtic ducts in a ſalmon.

29. The receptacle of the chyle and terminations of the lacteal and lymphatic ſyſtem in a ſmall ſalmon.

30. The lacteals of a turtle.

31, 32, 33. Certain ſpheroidal bodies connected with the brain, ſpinal marrow and the nerves of the cod and haddock.

34. The upper ſurface of the brain, and top of the ſpinal marrow of a ſkate, with the nerves iſſuing from theſe.

35. The noſe, ear, and larynx of a porpeſs.

36. The organ of hearing of the turtle.

The baſe of the brain of a ſkate, and the ſeveral parts of the organ of hearing.

38. The upper or back part of the head, and the ſtructure of the ear in the angel fiſh.

39. The optic nerves, and the ſeveral organs of hearing in the cod.

40. The noſe and ear of a ſturgeon.

40*. A ſection of the cryſtalline lens of the cod and the ox, with other circumſtances relating to the cryſtalline lens.

41, 42. The ſtructure of the cuttle fiſh.

43, 44. The parts, and particularly the abſorbent veſſels of the ſea egg.

From theſe contents, the reader may form ſome idea of the facts deſcribed by Dr Monro, and of the figures by which they are illuſtrated. And we ſhall next give a ſhort analyſis of the opinions which he founds on theſe.

After minutely deſcribing the heart, veſſels, and courſe of the blood in fiſhes, he deduces from thence ſeveral important obſervations. He remarks, that ſince the red particles of the blood are excluded from many parts, as in white fiſhes, from moſt of their muſcular fibres, which in the human body have numerous circulating arteries diſperſed upon them, it is evident, that in fiſhes there are numberleſs colourleſs arteries. Thus they afford clearer proof of numerous ſuch arteries than ariſes from the inſpection of the human body. Since from the circulating arteries of their muſcles, liquor muſt be ſecreted into the interſtices of the fibres, to prevent concretion, and to allow ready motion, proof he thinks is afforded, that ſecerning branches may be ſent off from colourleſs arteries, and that it is not neceſſary, on account of the want of impetus a tergo in colourleſs arteries, that ſecerning veſſels ſhould come directly from thoſe that convey red blood; and the exiſtence of a deſcending ſeries of arteries has been denied without good reaſon.

As there are red parts in fiſhes, and yet their venæ cavæ at the heart bear nearly the ſame proportion to the aorta or branchial artery as in man, we may, he thinks, conclude, that their colourleſs, as well as their red arteries, terminate in the venæ cavæ, and may from analogy infer, that the colourleſs arteries in the human ſpecies do not end in valvular lymphatic veins.

As the branchial arteries are divided into exceedingly minute branches, it may be concluded, that the force of the heart upon the blood will be very much loſt before that fluid gets into the branchial veins, eſpecially as no pulſation is diſcoverable in the branches of the aorta of a living ſkate. Hence he thinks we may infer, that the branchial veins which are of a thick and tough texture, and by their continuation form the aortic ſyſtem of arteries, are not fabricated in this manner merely to enable them to reſiſt the vis a tergo; but that theſe thick, tough and elaſtic coats are of a living and muſcular nature; and that the progreſſion of the blood through the reſt of the body of the fiſh, depends much upon their activity; and this is rather to be inferred, ſince a third circle is completed in their liver. Applying this to man, he concludes from analogy, that his arteries are of a muſcular nature, and that their activity is eſſential in circulation, ſecretion, and other important offices; and particularly, that the motion of the blood through the liver and ſecretion of the bile, depend chiefly on the muſcular ſtructure and action of the branches of the vena portarum.

In their abdominal venæ cavæ, and alſo between their venæ cavæ hepaticæ, and venæ cavæ, there are, he obſerves, large receptacles of blood, and the abdominal trunks of the cavæ, and ſeveral of their branches, are larger than their terminations. Theſe receptacles, he ſuſpects, may be neceſſary when they deſcend to a great depth. He imagines, however, that ſome other latent purpoſes are accompliſhed by this ſtructure; which he the rather concludes from the analogy of the lacteal and lymphatic ſyſtems, in which there are ſtill larger receptacles.

His laſt remark on this ſubject; is, that the circulation of the blood being carried on in the cartilaginous fiſhes, in the ſame manner as in the oſſeous or piſces of Linnæus, and the whole maſs of blood paſſing through the lungs, they muſt breathe regularly and uninterruptedly, to furniſh blood to the brain and other organs, and cannot poſſeſs the pulmo arbitrarius, as Linnæus and others have ſuppoſed. In the animals, commonly reckoned amphibious, he obſerves, that a part only of the maſs of blood paſſes through the lungs. Hence, in them, every part of the body may receive a conſiderable portion of blood, although reſpiration and the free paſſage of blood through the lungs be interrupted. Hence they are not under the ſame neceſſity with the mammalia, birds, and fiſhes, of breathing frequently regularly and alternately, but enjoy, he obſerves, the pulmo arbitrarius.

After treating of circulation, Dr Monro next proceeds to conſider ſome remarkable circumſtances relating to the glandular organs, and ſecreted liquors in fiſhes. After deſcribing a very elegant and pictureſque ſtructure of the organs which ſecrete the liquors that are poured out on the external ſurface of fiſhes, which had eſcaped former anatomiſts, but of which a proper idea can be formed only from viewing the plates, he remarks, that there appears

pears not only to be a very elegant ſtructure for the preparation of the mucus, but ſuch a ſudden change of the colour of nerve, as to give reaſon to infer that its continuation is not merely an expanſion of the matter of the brain, but that the ſtructure of the nerve is altered in its courſe. And it appears alſo, that ſome at leaſt of the organs of ſecretion are ſo far from being remarkable for the ſmallneſs of their nerves, that an uncommon ſhare of nervous energy ſeems neceſſary for them, to enable their veſſels to ſeparate and change the liquors they ſecrete.

After ſome obſervations on the watery fluid, impregnated with ſaline matter, found between the dura and pia mater in many fiſhes, and on the liquor found in the cavity of the abdomen, and deſcribing a communication, which he has diſcovered in the ſkate between the pericardium and the cavity of the abdomen, and in the ſturgeon, a communication between the cavity of the abdomen and the pelves of the kidneys, and, in both fiſhes, paſſages at the ſides of the anus, by which liquors may enter into or be diſcharged from the cavity of the abdomen, he gives it as his opinion that a great part of the latter fluid is taken from the ſea by holes at the ſide of the anus, of which he gives a particular deſcription. But he maintains, that a conſiderable proportion of it muſt be a ſecreted liquor, as it contains a much leſs proportion of ſalt than ſea water, and as a ſimilar degree of ſaltneſs is diſcoverable in the liquor in the cavity of the cranium, which ſeems to be entirely ſecreted from arteries.

With regard to the liquors ſecreted into the organs of digeſtion, he thinks it more evident than in man, that the gaſtric liquor acts as a menſtruum for their food. In all of them the liver is large, and of courſe the ſecretion of bile copious; and, in all, organs are found which pour out liquors ſimilar probably in their effects to thoſe of our pancreatic liquor; and by comparing different kinds of fiſhes with each other, and with animals of different kinds, he makes it very evident, that the bile is not derived from the coats of the gall-bladder, as the celebrated Albinus, and other authors had ſuppoſed, but that, in all animals, it is entirely derived from the liver.

After ſome obſervations on the ſtructure of the male and female organs of generation, he endeavours to refute an opinion lately propoſed, that the veſiculæ ſeminales are organs capable of ſecreting from their inner ſurface, a prolific liquor which is neceſſary to be mixed with that from the teſtes. And he concludes, that the veſiculæ ſeminales are receptacles for the liquor ſecreted by the teſticles, calculated to inſpiſſate the ſemen, and thereby render it fitter for its purpoſe, and, at the ſame time, to prevent copulation from being tedious.

In ſeveral kinds of fiſhes, Dr Monro has detected a hole or duct from the alimentary canal into the air-bag. But in others, as the haddock and cod, although the air-bag be very large, and its ſides remarkably ſtrong, he has not been able to diſcover any ſuch communication. In theſe fiſhes, a red-coloured organ, the ſurface of which is very extenſive, as it is compoſed of a vaſt number of leaves or membranes doubled, is found lining the inſide of the air-bag. But in thoſe fiſhes where the air-bag communicates with the alimentary canal, this body is either very ſmall and ſimple in its ſtructure, or entirely wanting. Hence, he thinks, there is ſome reaſon for ſuppoſing that the air is ſecreted by this body.

Dr Monro next gives a very minute and particular deſcription of the courſe and terminations of the lacteal and lymphatic veſſels, both in the nantes pinnati and piſces of Linnæus. This whole ſyſtem, he has found, terminates in all theſe nearly at the ſame places as in man, but as valves are wanting in the lymphatics of fiſhes, excepting at theſe terminations, he has been able to proſecute by injection the whole extent of the ſyſtem, and ſhews that its diſtribution is univerſal. Lymphatics in fiſhes are found in the brain and organ of ſenſe as well as in other organs, and the branches of this ſyſtem appear every where as numerous as the arteries. To this deſcription he ſubjoins ſome obſervations and experiments on the abſorbents of fiſhes. While, from a variety of obſervations and experiments, we can convince our reaſon, that the human valvular lymphatic veins are a ſyſtem of abſorbents, he contends, that in this claſs of animals we can go ſtill farther, and that deciſive ocular proof is afforded of the truth of the doctrine. Upon injecting the large lymphatic from the head and fore part of the ſkate, he found, that not only water, but even air, milk, quickſilver, or ſpirit of turpentine coloured with vermilion, were diſcharged from the ſurface of the ſkin, by a vaſt number of diſtinct orifices, placed at regular diſtances from each other.

From the ſmall force uſed in theſe injections, from the want of extravaſation in the cellular ſubſtance, and from the regular diſtribution of the orifices, he thinks there can be no doubt that theſe are the natural beginnings of the lymphatic veins, and as it is ſo very certain, that the lymphatic ſyſtem is abſorbent, that a vis a tergo is wanting, and as here no aſſiſtance is given by a valvular ſtructure, we muſt, he thinks, conclude, that the coats of the lymphatic veſſels are of an active or muſcular nature. To his general deſcription of the lymphatics, he ſubjoins a chapter on the lymphatics and uſes of the ſpleen, in which he endeavours to prove, that the opinions of Mr Hewſon and Mr Falconer on this ſubject, are not well founded. His obſervations on this ſyſtem of veſſels are concluded by a vindication of his claim to the firſt diſcovery of lacteals and lymphatics in fiſhes, birds, and amphibious animals.

After the lymphatics, Dr Monro next treats of the brain and organs of the ſenſes in fiſhes. The brain, he obſerves, is ſenſibly ſmaller in proportion to their bodies, than in the mammalia, or birds. Yet the nerves it ſends off are as large in proportion to the ſeveral organs as in theſe two claſſes. In all fiſhes, external openings for ſmell are, he obſerves, very evident. And in ſome, the olfactory nerve, in its courſe from the head to the noſe, paſſes through a cineritious ball, reſembling the cineritious matter in our body, connected to the olfactory nerve within the cranium. There can, therefore, he thinks, be no doubt that they enjoy the ſenſe of ſmelling; and there is great reaſon to believe, that they are much more ſenſible of odorous bodies diſſolved in water, and applied by its medium, than we ſhould be if the application of the object was to be made to our organ of ſmell by the ſame medium.

After a very minute account of the anatomy of the ear in the cetaceous fiſhes, in amphibious animals, particularly in the ſea tortoiſe, or turtle, in the nantes pinnati or cartilaginous fiſhes, particularly in the ſkate or raja, and the ſqualus ſquatina or angel fiſh, in both which laſt he has diſcovered meatus auditorii externi, and alſo in the oſſeous fiſhes, and chiefly in the cod, he relates ſome experiments on hearing in water. From theſe he found, that although ſound was ſenſibly leſs acute in water, yet that it powerfully affected the ear, and was very quickly conveyed. Nay, from ſome experiments which he deſcribes, there is reaſon to ſuſpect, that it may be as quickly, if not more quickly, conveyed by water than by air; and he deſcribes a method in which he thinks this very curious point may be determined. To theſe deſcriptions and experiments he ſubjoins a chapter on the ſeveral ways in which the tremor of ſonorous bodies is communicated, in the different claſſes of animals, to the nerves ſpread out on the bottom of the ear. In this, among other obſervations, he mentions an experiment which renders it very doubtful, whether ſound be conveyed by the human euſtachian tube, as has generally been ſuppoſed.

In treating of the eyes of fiſhes, he remarks ſeveral curious facts concerning their coats; and in particular, he obſerves, that in the white rabbit the pigmentum nigrum is entirely wanting, and that in all animals the iris circulates a great quantity of red blood, inſtead of being furniſhed with colourleſs arteries only, which was taken for granted, by the modern anatomiſts, on the authority of Ferrein and Haller. He next conſiders their humours, treating of their texture, ſpecific gravity, weight, ſhape, and powers of refraction. He ſhews clearly, that the ſhape of the cryſtalline lens in man and other animals had been very erroneouſly deſcribed by Dr Petit, whoſe facts on this ſubject had ſo much the appearance of accuracy as to be univerſally received.

From his obſervations on theſe ſubjects, he concludes, that, to enable fiſhes, with the ſame length of the axis of the eye as in quadrupeds, to collect into a focus on the retina, the rays of light coming from the denſe medium of water, four circumſtances chiefly concur: *1ſt*, Their cryſtalline lens is more convex, or compoſed of portions of ſmaller ſpheres than in land animals; *2dly*, Their cryſtalline lens is, in correſponding parts, much more denſe than in animals which live in air; *3dly*, The lens in fiſhes poſſeſſes powers of refracting light far beyond what has been calculated by authors who have proceeded on the ſuppoſition that theſe powers are proportioned nearly to its ſpecific gravity; and, *laſtly*, The vitreous humour of fiſhes being lighter than that of land animals, the rays of light iſſuing from their lens will be refracted in a greater degree, or brought ſooner to a focus. He concludes his remarks on this ſubject with obſerving, that attention to the denſity, ſhape, refracting powers, and connection of the humours of the eyes of animals, may lead to the ſtill farther improvement of optical inſtruments.

He ſubjoins as an appendix to this work on fiſhes, a deſcription of the ſtructure and functions of two very curious animals, the ſepia loligo, or cuttle fiſh, and the echinus marinus, or ſea egg. The former of theſe is not leſs remarkable in its internal ſtructure than in its external ſhape. For, beſides its very ſingular appendix of an ink-bag, it has been found, by our author, to have three hearts for circulating its blood. And both theſe animals poſſeſs the very curious power of attaching themſelves to contiguous bodies, ſomewhat in the ſame way as a boy attaches a piece of wetted leather to a ſtone.

But, in the echinus, each of the organs by which it ſecures its prey is perforated, and the hole leads to a muſcular tube, which, after piercing the ſhell of the animal, divides into a very curious net-work of minute veſſels, intended for the abſorption of the ſea water, and its ſecretion within the cavity of the ſhell, where it is lodged in conſiderable quantity. This very beautiful and ſingular ſtructure, which is accurately proſecuted by our author, after he had filled it with quickſilver, afforded him an opportunity of making experiments with the view of determining the manner in which fluids are taken in and conveyed by abſorbent veſſels. But for a full account of theſe we muſt refer to the work itſelf, which we conſider as no leſs meriting the attention of the phyſiologiſt than of the natural hiſtorian.

XIV.

Obſervations on the Animal Œconomy, and on the Cauſes and Cure of Diſeaſes. By John Gardiner, *M. D. Preſident of the Royal College of Phyſicians, and Fellow of the Royal Society of Edinburgh.* 8vo. Edinburgh.

THE work before us contains many obſervations which, whether they be well founded or not, will at leaſt be generally allowed to be new and ingenious, and may, we think, lead to uſeful improvements in practice. It is divided into nine ſections; and, as Dr Gardiner imagines that the improvement of medicine depends more on extending our knowledge of the animal œconomy, than on any other circumſtance, he treats, in the firſt ſection, of the living principle in animals. He preſents us with ſome intereſting obſervations concerning its power, or rather the power of the body which it animates, in regulating the animal heat, when the body is expoſed to degrees of heat either above or below the natural ſtandards.

He contends, that it poſſeſſes a power of reſiſting the effects of heat and cold to a certain degree; but that, when the body becomes a few degrees warmer or colder than its natural ſtandard, the principle of life is proportionally weakened, and at length totally extinguiſhed. He endeavours to ſhew, that the principle of life is diffuſed through the whole ſolids and fluids of the body. But he conſiders it as being chiefly ſeated in the brain, cerebellum, and their appendages, from whence it is conducted to all parts of the body by the nerves.

He obſerves, that the nerves conduct the powers of action with more facility during the growing ſtate of the body, than in manhood, when our motions are leſs alert, but more firm. This inaptitude of the nerves to conduct the powers of motion, increaſes, he thinks, as age advances, that faculty continuing to diminiſh, with a ſlow but certain pace, till at laſt the nerves become unfit to convey a ſufficiency of the powers of life for the continuance of the functions. Then diſeaſe muſt enſue, and death be the conſequence.

In this ſection are contained ſeveral intereſting obſervations on the limitations of the powers of action, on the effects of paſſions, on the ſympathy between the heart and lungs, on the influence which hard labour and ſtudy have on the body, and the like. In concluding it, he contends, that a portion of the living principle is not only preſent in every part of the body, but remains in it for ſome time after death. He conſiders heat and moiſture as being neceſſary for maintaining the powers of the living principle. And he is of opinion, that, previous to death, in conſequence of certain fevers, there is a gradual exhauſtion of the principle of life in every part.

In the ſecond ſection he treats of the nerves, of ſympathy, and of ſtimuli. And here, though the ſubject, it muſt be allowed, is of a conjectural nature, yet his opinions appear to us to have much merit. He explains the variety of ſenſibility and irritability in different parts of the body, from the manner in which the nerves terminate, or from the particular apparatus given them at their termination, to fit them for their different offices in the ſyſtem. From this variety of conſtitution, given to the nerves in different parts of the body, he is of opinion, that certain ſtimuli, applied to the ſtomach or bowels, are apt to affect particular organs, and are therefore called ſpecific. But he conſiders general ſympathy, with a particular part, as ariſing from that unity of ſubſtance which is known to exiſt between the brain and nerves. In this ſection, we are preſented with many obſervations on the effects of noxious and ſalutary ſtimuli applied to the body, in the production of diſeaſe, and in the reſtoration of health; on the manner in which vegetable poiſons operate on the body; on the effects of pain; on the palſy; on the unequal diſtribution of the powers of action in diſeaſes of the mind, as well as in thoſe of the body; on the action of what he calls inconſcious ſtimuli, from which are derived the good effects of moſt medicines, as well as the noxious effects of the general cauſes of fever. His reflections on theſe ſubjects, of which we cannot here give a more particular account, ſhow much judgment and ingenuity.

The ſubject of the third ſection is an enquiry concerning the effects of heat and cold; or, as it might, perhaps with equal propriety, have been entitled, an enquiry into the origin of animal heat, and the power which the animal body poſſeſſes of reſiſting the effects of heat and cold, when placed in an air heated above, or cooled below its temperature. He is diſpoſed to adopt Dr Crawford's theory, with reſpect to reſpiration; but he differs from him with regard to the manner in which animal heat is produced.

According to Dr Crawford's view of animal heat, it depends on a proceſs ſimilar to a chemical elective attraction. The air is received into the lungs, containing a great quantity of abſolute heat, and the blood is returned to them from different parts of the body, highly impregnated with phlogiſton. The attraction of air to the phlogiſton, aſſiſted by the heat of the lungs, is greater than that of the blood. This principle, therefore, leaves the blood to combine with the air. By the addition of phlogiſtod, the air is obliged to depoſit a part of its abſolute heat; and as the capacity of the blood for receiving heat is, at the ſame inſtant, increaſed, by the ſeparation of phlogiſton, it will immediately unite with that portion of heat which had been detached from the air. But the blood, in the courſe of circulation, abſorbing phlogiſton, and thereby having its capacity for containing abſolute heat diminiſhed, part of it, in proportion to the quantity of phlogiſton abſorbed, breaks out in the form of ſenſible moving heat, and is, according to Dr Crawford, the principal ſource of animal heat.

This opinion is, our author thinks, more probable than any that has been given by former writers. But, notwithſtanding his partiality to this theory, yet he cannot altogether aſſent to Dr Crawford's reaſoning with regard to the manner in which animal heat is ſupported. For, allowing it to be true, that, when phlogiſton is abſtracted from any body, its capacity for containing abſolute heat is thereby increaſed, he thinks it is difficult to perceive how the abſolute heat, abſorbed by the blood from the air inſpired, ſhould become the ſource of animal heat, in the courſe of circulation. For, in the double exchange of phlogiſton from the blood for fire from the air, in reſpiration, no increaſe of heat is raiſed in the lungs; becauſe it is ſuppoſed, that, as the capacity of the air for containing abſolute heat is diminiſhed on receiving phlogiſton, the capacity of the blood is proportionally increaſed in parting with it. But why, ſays he, does not the ſame reaſoning apply to the abſorption of phlogiſton by the blood, in the courſe of circulation? For the fluid or ſubſtance which parts with its phlogiſton, ought inſtantly to have its capacity for containing abſolute heat enlarged, and, of courſe, ſhould receive it as a principle in its compoſition.

Our author ſuppoſes, that ſuch a double exchange of principles as is mentioned by Dr Crawford, does take place in reſpiration; and that part of the fire extricated from the air is expended in converting a quantity of moiſture into vapour, but that the remainder is abſorbed by the blood. And this fluid being neceſſarily kept in that degree of heat peculiar to the animal, and having ſeveral fluids ſecreted from it, different in their nature from each other, and ſome of them in conſiderable quantity, he thinks it is reaſonable to ſuppoſe, that ſuch a change takes place from the ſtate it poſſeſſed in the large arteries, as leſſens its capacity for containing abſolute heat, and that, of courſe, there will be a gradual extrication of part of the fire it held as a principle, into active meaſurable heat. This theory, he thinks, is confirmed by Dr Crawford's experiments, which ſhew, that the capacity of the arterial blood, for containing abſolute heat, is to that of the venous as 115 to 100. Conſequently, the ſame degree of heat which would raiſe the venous blood to 115, would raiſe the arterial only to 100, the difference being abſorbed by the latter in the form of abſolute heat.

To this theory of animal heat, our author ſubjoins many intereſting obſervations on the power which animals poſſeſs in reſiſting the effects of heat and cold, and on the diſeaſes ariſing from exceſs of theſe. But as a proper account of theſe would extend our analyſis beyond due bounds, for his ſentiments on theſe ſubjects we muſt refer the philoſophical reader to the work itſelf.

In the fourth ſection, Dr Gardiner treats of fevers in general. The cauſes of theſe he comprehends under the following heads: exceſs of cold, exceſs of heat, marſh miaſma, human contagion, and ſpecific contagion. He obſerves, that although each of theſe cauſes may act ſingly on the body, in the production of fever, yet that they are in many inſtances combined, and from that combination produce the great variety of fevers obſerved in practice.

After taking a view of the different ways by which infection is ſuppoſed, by authors, to enter the body, by the pores of the ſkin, by reſpiration, by inoculation, or by mixing with the ſaliva, he attempts to refute the three former, and gives many remarkable proofs of its being received, in general, into the ſtomach by the ſaliva, where it acts as a ferment, and produces ſome changes in the ſecretions in the primæ viæ, previous to the acceſſion of fever. He does not indeed deny, that infection may be received by inoculation, and gives ſome judicious obſervations on this method of communicating the ſmall-pox. But he contends, that this cannot be the general manner in which infection is received in fevers.

The ſubject of his fifth ſection is ſimple catarrh. Here he attempts to refute the common opinion, of its ariſing from a ſtoppage of perſpiration, founding his arguments on the ſtatical obſervations and experiments of Sanctorius and Keil. From their authority, and indeed from daily obſervation, it appears, that perſpiration may be conſiderably diminiſhed without producing any catarrhal ſymptom. He denies that a ſtoppage of perſpiration takes place either during the continuance of a catarrh, or at its firſt acceſſion; and it is in favour of this refutation of the common opinion, that Dr Keil, during his ſtatical experiments, was twice ſeized with a catarrh, without having his perſpiration in the ſmalleſt degree diminiſhed.

He deduces the cauſe of catarrh, from a ſympathy between the ſkin and the mucous glands of the internal membrane of the bronchiæ, noſe, fauces, and neighbouring parts. He takes notice of the ſalutary effects of cold in bracing the ſolids, and of the aſſiſtance it gives in regulating animal heat. But he alleges, that when the cold, applied either in a general way, or to any particular part of the body, is ſo intenſe as to diminiſh the power of the nerves, for any conſiderable time, then a morbid condition takes place in the parts affected, which, by ſympathy affects the mucous glands mentioned above, more readily than any other part of the body, becauſe they are leſs capable of reſiſting the particular ſtimulus of cold: and he gives ſeveral examples, to ſhew that cold is, in general, ſpecific with regard to its effects upon the lungs. But, at the ſame time, he obſerves, that when, on account of ſome former diſorder, as the gout, rheumatiſm, diarrhœa, fits of calculus, or the like, certain parts of the body have become more irritable than the reſt, it is not the common catarrhal ſymptoms which are the conſequence of ſuch perſons being expoſed to cold, but a return of their former diſorders.

In the ſixth ſection of this work, our author treats of what he ſtyles the catarrhal fever. This he conſiders as being a diſeaſe of a middle nature, between a catarrh and an inflammatory fever; and he holds it to be the product of cold. For, ſays he, when cold operates on the body ſo as to induce ſome morbid change, it is the arterial ſyſtem and mucous glands that are chiefly affected. But this operation of cold may proceed, for ſome time, before the ſyſtem be ſenſibly affected by it. For, till a preternatural irritability of the body be induced, he contends, that a remarkable change in the functions will take place. But when this irritability is raiſed to any conſiderable degree, then a collection of phlegm in the ſtomach and bowels, from a diſeaſed and increaſed ſecretion of their glands, acts as a febrile ſtimulus on the ſyſtem, and brings on ſome degree of coldneſs, ſhivering, and other ſymptoms, which commonly attend the acceſſion of ſuch fevers. Theſe ſymptoms, he thinks, ariſe from a general ſympathy of the body, and particularly of the ſkin, with the primæ viæ. But as, from the commencement of the acceſſion, a conſiderable thirſt takes place, accompanied with a remarkably quick abſorption from every cavity of the body, he ſuppoſes the phlegm and acrid fluids, in the alimentary canal, to be ſoon abſorbed, and carried into the circulation, by the lacteals. Then the ſymptoms of the cold ſtage ceaſe, becauſe there is not a ſufficiency of the morbid fluids left in the primæ viæ to continue theſe effects. But the diſeaſed ſtate of the ſecretions, and preternatural irritablity of the body, ſtill ſubſiſting, the fever continues for an indeterminate time, or, till theſe morbid conditions of the body be rectified.

From the view which our author takes of the cauſe of the cold ſtage in fever, he concludes, that it is not eſſential to fever, and far leſs can be conſidered as its cauſe. For he conſiders a certain quantity of morbid fluids, in the ſtomach and bowels, as neceſſary to produce a cold ſtage; and this is not always the caſe at the commencement of fevers. In proof of this doctrine, he obſerves, that there are many inſtances in which fevers come on without any cold ſtage. This often happens, even in intermittents, and eſpecially when the primæ viæ have been emptied by an emetic, previous to the time of the acceſſion.

Dr Gardiner has never been able to obſerve that regularity in the critical days ſo ſtrenuouſly inſiſted on by the ancients, and by many of the modern authors; and he affirms it, as an undeniable fact, that the period of a fever, is ſhortened or protracted by a good or bad practice. He believes, that the materies morbi is generated chiefly during the courſe of the diſeaſe, but that the critical diſcharge of it does not happen till the morbid ſecretions in the alimentary canal and mucous glands are rectified, and the preternatural irritability of the ſyſtem ceaſes, by which a ſtricture on the ſecretory veſſels of the kidneys, and other organs, is taken off.

After the view taken in the preceding ſections, of the effects of exceſs of cold on the human body, in the production of diſeaſes of an inflammatory tendency, he proceeds to conſider ſome of the effects of an exceſs of heat; and, firſt, he treats of cholera morbus, which is the ſubject of his ſeventh ſection. This he does not conſider as a febrile diſeaſe, but as a diſorder connected with the bilious remitting and intermitting fevers of autumn in Europe, and of all ſeaſons of the year in warmer climates, particularly within the tropics. This diſeaſe, our author ſuppoſes, with other writers on the ſame ſubject, to ariſe from the heat of the ſummer or climate. This heat, according to its degree, has, he thinks, a proportional influence in increaſing the ſecretion of bile, which accumulates in the inteſtines, and, becoming acrid by ſtagnation, produces the diſeaſe.

In the eighth ſection, where our author treats of the bilious intermitting and remitting fever, he gives a different account of the nature and cauſes of this diſeaſe, of its various forms, and of its changing from one form to another, from what we have hitherto met with. The moſt ſimple form of this diſeaſe, according to Dr Gardiner, is the inflammatory ſpecies, which is, he thinks, compounded of a catarrh or catarrhal fever, with cholera. He obſerves, that when marſh miaſma has a conſiderable ſhare in the production of this diſeaſe, it diſpoſes to intermiſſions;

termiſſions; but that human effluvia, or the foul air of an hoſpital, gives a tendency to a continued fever; and, laſtly, that ſpecific contagion, the vapours from privies, cold, and peculiarity of conſtitution, diſpoſe the ſame diſeaſe to the form of dyſentery.

As the dyſentery often changes into an intermittent, and this into a remittent, at laſt taking the form of a continued fever; and as theſe changes ſometimes run in a contrary way, it ſhews, he thinks, that the diſorders, with reſpect to their cauſes, are the ſame. He is of opinion, that the principal difference ariſes from the prevalence of ſome one of the general cauſes of fever, the degree of heat, or moiſture, to which the ſick may have been expoſed, the ſtrength of the perſon to reſiſt the power of theſe noxious vapours, a peculiarity of conſtitution, and ſimilar circumſtances. He contends, that the acceſſion in this fever is more ſtrongly marked than in the catarrhal fever, on account of the greater quantity of acrid bile in the primæ viæ acting as a febrile ſtimulus on the ſyſtem. In like manner, he conſiders the remiſſions as being more complete in this than in any other of the inflammatory fevers, on account

count of the bile, and other acrid fluids, being in a more fluctuating ſtate with reſpect to their quantity.

He is of opinion, that the delirium, with which ſome perſons are ſeized at the commencement of this diſeaſe, is owing to bile and phlegm in the primæ viæ, and that it is moſt effectually removed by vomiting and purging. On the ſubject of blood-letting in this fever, he contraſts the obſervations of Dr Monchy, phyſician to the Dutch forces, who never permitted it, with thoſe of Sir John Pringle, who ſeldom omitted it. In moſt caſes, our author diſapproves of veneſection. In ſome few caſes, where the effects of an exceſs of cold to the body appear to have had a conſiderable ſhare in the production of the diſeaſe, he conſiders blood-letting as indiſpenſably neceſſary. But in the malignant kind of this fever, which chiefly ariſes from the vapours of marſhes and fenny grounds, he conſiders it as ſeldom admiſſible; and where, on account of ſome urgent ſymptom it is thought adviſable, it ought to be performed, he thinks, with great caution; the leaſt of its bad conſequences being that of protracting the diſeaſe.

In his obſervations on the uſe of Dr James's powder in this fever, as a means of clearing the ſtomach and bowels of their acrid contents, he takes occaſion to remark, that the calx antimonii nitrata of the Edinburgh Pharmacopœia has been ſuppoſed to be a ſimilar medicine. But he has found, that five grains of James's powder are equal to eight or ten of the calx. From different circumſtances, he was led to imagine, that Dr James mixed a certain proportion of tartar emetic with his prepared calx. And, he has diſcovered, that, if one grain of the emetic tartar, prepared according to the Edinburgh Pharmacopœia, be mixed with twenty grains of their calx antimonii nitrata, by rubbing them well in a glaſs mortar, its operation, and ſtrength, are very nearly ſimilar to thoſe of the celebrated powder of Dr James.

In his ninth and laſt ſection, he treats of the intermittent fever. And here, alſo, he gives an explanation, different from other authors, of the phænomena of this diſeaſe. It is allowed by all medical writers, and is confirmed by common obſervation, that this diſtemper is moſt frequent in the neighbourhood of marſhy and fenny grounds; and this has, with great probability, been ſuppoſed to ariſe from the unwholeſome vapours of ſuch ſituations. But very different accounts have been given as to the manner in which theſe noxious exhalations affect our ſyſtem. Our author ſuppoſes, that they are taken in with the air in reſpiration; that they mix with the ſaliva, and with it are carried into the ſtomach. He ſuppoſes, that, in the ſtomach, according to their malignancy, they produce a gradual or quick change in the ſecretions. The conſequences of this morbid affection and ſecretion of the glands in the alimentary canal, are, in many caſes, not remarkable for ſome time. There is only obſerved a want of appetite, and paleneſs of the complexion, and, at times, a ſlight degree of laſſitude, until the bile, and other fluids, rendered acrid by ſtagnation, are accumulated in ſuch quantity as to produce a cold ſtage. At this time there is often vomiting of bile and phlegm, and ſometimes looſeneſs. But a conſiderable quantity of the acrid fluids being, by dilution and abſorption, carried into the circulation, the cold fit ceaſes, and the hot commences, with ſome irregular returns of ſhivering, occaſioned, he thinks, by a quantity of morbid fluids remaining in the primæ viæ, the farther abſorption of which goes on during the hot fit.

At this period, there is a conſiderable fulneſs, and every ſymptom of plethora in the ſanguiferous ſyſtem. But, as there is not, at laſt, a ſufficiency of acrid fluid in the ſtomach and bowels, to act as a febrile ſtimulus, relaxation with a critical ſweat takes place, and continues for ſome hours, leaving the patient in a ſtate of apyrexia. But, as he ſuppoſes the diſeaſed condition in the liver and glands of the primæ viæ ſtill to ſubſiſt, and as Nature is uniform in her operations, he explains the return of the paroxyſms within a certain period, from the ſame quantity of morbid fluids being again collected, in the ſpace of twenty-four, forty-eight, or ſeventy-two hours.

When our author treats of the cure of intermittents, he offers many important obſervations on the uſe of emetics, laxatives, bitters, aſtringents, cordials, and the Peruvian bark. And from a conſideration of the operation and effects of theſe medicines, he thinks, that the ſtrongeſt evidence is afforded of the truth of his doctrine.

SECT. II.

Medical Obſervations.

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I.

Obſervations on Dropſies prevailing among the Troops in the Eaſt Indies. By Mr William Dick, *Surgeon of Artillery, Bengal Eſtabliſhment, in a Letter to* Dr Duncan, *from Madraſs.*

ABOUT ſeven years ago, I had the honour of attending your lectures; and, ſince that time, have gained ſo much uſeful knowledge from your different publications, that I ſhall eſteem it a particular happineſs, if, at any time, I can make obſervations on the diſeaſes of this country, which may contribute towards your private amuſement, or deſerve a place in your Medical Commentaries.

I have been now about two years with the army in the Carnatic, and having the care of 300 artillery men, (a detachment from the Bengal eſtabliſhment) I had an opportunity of obſerving the diſeaſes to which ſoldiers are moſt liable, when campaigning on this coaſt. But as my intention is here to give you an account of the dropſies which proved ſo fatal to the troops in cantonments, during the rainy months of October, November, and December, I ſhall only mention the reſt in a curſory manner.

In April, May, June, and July, the land winds blow ſo exceedingly hot and dry, that life is hardly ſupportable at noon. The cholera morbus, dyſenteries, inflammations of the liver, and ardent, or what they call bilious fevers, become frequent in camp at this ſeaſon. A ſpecies of apoplexy, which ſeizes the men when fatigued by marching in the heat of the ſun, proves, however, more fatal to the Europeans than any of the above. They complain firſt of a great headach, thirſt, and ſome difficulty of breathing; in a few minutes, a vertig, and bilious vomiting come on, they drop down breathleſs, turn comatoſe, and unleſs immediate aſſiſtance be given, the face ſwells and turns almoſt black; the pulſe, which was at firſt full and quick, ſinks; and after ſome hard ſtruggles for breath, they expire. Removing them under the ſhade of tree, bleeding them freely in time, and giving them ſome water, generally cures them; but as the ſtomach and bowels are often overloaded with bilious or putrid matter, it is neceſſary in the evening, to give them ſmall quantities of tartar emetic diſſolved in a ſaline mixture, which anſwers better than any other evacuant. Though they ſeldom require any other medicine, yet they are a long time unfit for duty, after an attack of this kind.

The next moſt frequent and fatal diſeaſe is the cholera. The attack is often ſudden, and always violent. The ſymptoms are a vomiting and purging of quantities of fluid matter; ſometimes dark, often colourleſs, but moſtly of the different ſhades between a light yellow and dark green. It often ſtains linen of a graſs colour, excoriating the throat and mouth; and there is moſt danger when this kind is vomited. Severe griping torments the patient;

and, unleſs the diſeaſe is quickly ſtopped, the pulſe becomes weak, quick and irregular; cold ſweats break out; a burning heat at ſtomach; great thirſt and anxiety come on. The cramp, which at firſt attacked the extremities, now ſeizes on the ſtomach and belly. The hippocratic face, hiccough, and coldneſs of the extremities, which ſoon ſucceed, point out the approaching diſſolution. A man taken ill in this manner, ſeldom lives above ten or twelve hours. If the cramp ſeizes on the vital organs at firſt, he ſeldom ſurvives it three hours. When I am called to ſee a patient, at firſt I make him drink as much warm water as he can ſwallow; and after his ſtomach is waſhed, glyſters of it are thrown up till the griping ceaſes, or the colour of the offending matter is changed. An infuſion of linſeed is by that time ready, and fifty or ſixty drops of laudanum added. This is given as a glyſter; a cordial draught, with a few drops of laudanum, or a grain of opium, is then ſwallowed, which compoſes the patient, and a little rhubarb next morning generally finiſhes the cure. In ſome caſes where the cramps were violent, I have uſed a liniment made with opodeldoc and laudanum, and having it well rubbed with a warm hand on the belly and limbs, it produced the happieſt effects. Though medicines are not neceſſary after this, yet, ſuch a degree of weakneſs remains, as renders the free uſe of cordials and light nouriſhing diet proper for ſome time. Even in the worſt caſes, I believe, the warm bath would remove the cramp; but we could not avail ourſelves of that advantage in the field; and I never ſaw any good from fomentations. Perhaps rolling the patient in a blanket, and putting a number of pots full of boiling water under his cott, might, by the ſteam, anſwer the ſame intentions. But want of theſe things in the field, prevented me from trying the experiment.

When I was ſurgeon of the Queen Indiaman, we arrived in Madras roads in company with another ſhip, at the time the land-winds blew hotteſt. Many were ſo ſeverely attacked with the above complaint, that I was under the neceſſity of keeping a caſk full of warm water always ready in the galley to put the men into, as ſoon as the cramps ſeized them. After this, they were rolled in a blanket, and plenty of warm wine and water given as ſoon as the ſtomach was ſufficiently ſettled, and every one of them recovered. Before the ſame method was adopted in the other ſhip, ten or twelve of their beſt men died the firſt week. The natives of this country are alſo ſubject to this diſeaſe, particularly in September and October, when the nights begin to turn cold; and as they ſeldom apply immediately for aſſiſtance, few of them recover. The vomiting and purging are not ſo ſevere; but their pulſe ſinks immediately as the cramp comes on; and as they ſeldom have much clothes to cover them, and but the bare ground for a bed, it is hardly poſſible to cure them. Strong frictions, with hot coarſe clothes, hot ſpirituous embrocations, and plenty of hot wine and ſpices, are the only things I ever ſaw of uſe to them.

Dyſenteries are not ſo frequent here as in many other parts of India; and, when they do appear, they are often the effects of liver complaints, and, as ſuch, are treated with mercurials and repeated purges, to carry off the bile, as they term it: I wiſh I could add that ſucceſs, in general, attended this practice. Liver complaints, and a vitiated ſtate of the bile, or a redundancy of it at leaſt, are undoubtedly frequent here; and therefore an opinion prevails, that every other diſeaſe is cauſed by, or connected with them, and mercury is of courſe uſed in every diſeaſe, almoſt without exception. I have had an opportunity of attending numbers ill of the dyſentery at Batavia, Bencoolen, Bengal, and here, and have opened many that died of it. But as I have not time to ſay much about it now, I wiſh only to remark, that I have ſeen ſuch good effects from ipecacuanha, given in ſmall doſes, in caſes where there were a feveriſh heat, quick weak pulſe, foul tongue, and the patients complained of nauſea, and much griping, that I am aſtoniſhed how Dr Clark came to condemn it as an uſeleſs drug. He certainly muſt have met with it very bad, to induce him to give us ſuch an account of it. I have generally given an infuſion of it, and found, though it vomited the patient the firſt day, it ſeldom had that effect afterwards; and, in general, it occaſioned more copious evacuations of excrementitious matter than many of the ſtronger purges. All medicines for this complaint ought to be given in a fluid ſtate, if poſſible, as I find they irritate and gripe leſs than powders, however fine they may be. And I am convinced of no fact more, than that all oily medicines are hurtful, and only render the uſe of emetics more neceſſary than the irritable ſtate of the ſtomach always admit of. I believe alſo, that a more ſparing uſe of purgatives, and a freer uſe of glyſters, would in general anſwer better.

I have not had ſufficient experience to be able to give you an account of the beſt, or moſt ſucceſsful method of treating diſeaſes of the liver. Mercury has done a great deal of good in particular caſes, when ordered by the judicious practitioners here; and therefore is preſcribed as a certain ſpecific by the reſt in almoſt every caſe indiſcriminately. I need hardly mention, that I have ſeen it produce the worſt effects, when given to a plethoric patient, who is ſeized with an inflammation of the liver, and where only copious bleeding, ſaline medicines, glyſters and bliſters, would have prevented the ſuppuration, or even the mortification that followed. Nor can it be ſuppoſed to have any very good effects, after matter is formed in any quantities in the liver; for unleſs the ſuppuration points outwardly, ſo as to admit of being opened, it either burſts into the lungs, cavity of the abdomen, or inteſtines, and the patient generally dies hectic, or of a dyſentery, whether mercury be uſed or not. It is only where there are evident ſymptoms of obſtruction and enlargement remaining, after the inflammation has gone off, where the quantity of matter is very trifling, and in chronic caſes, that mercury ever does good. From the frequency of liver complaints on this coaſt, and the ſucceſs with which the learned and ſkilful Dr Anderſon, ſurgeon-general of this place, has practiſed for upwards of twenty years, it is greatly to be lamented that he does not favour the world with a treatiſe on it. No other man alive is perhaps poſſeſſed of equal experience and abilities to do it.

I ſhall now give you an account of the dropſies which proved ſo troubleſome in cantonments, in the latter end of 1782 and 1783, when the monſoons were uncommonly wet and ſevere. The men, from being accuſtomed to violent exerciſe and fatigue for nine months of the year, were at once ſent into quarters, where they ſank into a ſtate of indolence and inactivity. No cots being allowed, what little bedclothes they had were laid on the damp floor of the barracks. By this, and the conſtant moiſture of the air, the perſpiration was entirely ſtopped, and agues, colds, and dropſies were the conſequences. The firſt year I had fifty patients with anaſarcous ſwellings. Many of them were greatly ſwelled all over the body; yet four or five only had their breathing much affected; and, of the whole, two died. The next year, many of the ſame people had relapſes; yet none that were taken ill had ſo much ſwelling of their limbs as the year before. They were chiefly affected that ſeaſon in the following manner: they complained firſt of an unuſual languor, weakneſs, ſtiffneſs and pains in their joints, hardneſs in the muſcles of the thighs and legs, and ſome ſwelling of their ancles at night. Loſs of appetite, coſtiveneſs, and ſcarcity of urine, together with irregular heats ſucceeded. In this manner they continued for ſeveral days, without applying for aſſiſtance, till, upon motion, their breathing became more or leſs affected. By this time, the feet and legs began to continue ſwelled the whole day; and ſoon after, the breaſt, neck, arms, and hands, became œdematous in the night-time, while the face always appeared bloated, puffed up, and of a tallow or leaden colour; horrid dreams, and frequent ſtartings, diſturbed their reſt; the oppreſſion at breaſt gradually increaſed, till they could only ſleep in a ſitting poſture; a troubleſome cough, often with ſtrong palpitations of the heart, diſtreſſed them greatly; the epigaſtric region turned hard and ſwelled, and an acute pain was felt conſtantly at the pit of the ſtomach. They compared the load at breaſt to a weight of lead laid on the ſternum; and any conſiderable motion threatened immediate ſuffocation. The ſkin turned cold and clammy, the pulſe irregular, the ſwelling of the legs ſubſided, the tongue turned dry, the thirſt unquenchable, the anxiety and pain at ſtomach inexpreſſible; and they found relief from nothing ſo much as drinking large quantities of cold water, which was immediately vomited up again. After the vomiting came on, they generally lived eight or ten hours; and all that died in this manner, died in leſs than a month from their firſt feeling themſelves indiſpoſed; and generally in leſs than eight days time after the oppreſſion at breaſt came on.

The diſeaſe did not always go on in this gradual manner. In ſome the ſymptoms of oppreſſion were ſlight, and no danger apprehended till a day or two before death, when the ſwelling of the legs diſappeared ſuddenly; the difficulty of breathing, tenſion, and ſwelling of the epigaſtric region, thirſt, and vomiting, as ſuddenly came on, and hurried the patient out of the world when he leaſt dreaded it.

From the ſymptoms, the ſeat of the water was eaſily known. When effuſed in the thorax, it ſeldom occupied both cavities; and as the patient could only ſleep on the diſeaſed ſide, the face, neck, arm, hand, and cellular ſubſtance over the ribs of that ſide, were always more ſwelled than on the other. The patient could ſometimes ſleep with the head very low; and though a cough frequently attended, it was not very troubleſome; and motion did not occaſion ſuch uneaſineſs in breathing as in the following caſes.

As to fluctuation, though I have heard it, yet I am almoſt certain the fluid was in the ſtomach; and perhaps it is a difficult matter for the moſt ſkilful to diſtinguiſh the difference. It was only by jolting the patient, when laid on his back in a palanquin, that I could plainly hear a fluctuation, in any caſe, though I took a good deal of trouble in trying, by putting the patient in different poſtures.

When the water occupied the cellular ſubſtance of the lungs, the ſymptoms of oppreſſion were more ſudden; the leaſt motion threatened ſuffocation; the pain and weight complained of were exactly felt under the ſternum; the ghaſtly look, anxiety, and reſtleſſneſs, were greater; the pulſe was quicker and weaker; the cough more troubleſome, and often attended with a conſiderable expectoration of a frothy white fluid, reſembling the ſcum of new milk, and which, in ſome caſes, was poured into the bronchia in ſuch quantities as ſuffocated the patient. When there was a neceſſity for lying down, it made no difference whether he lay on his back or his ſides; but, for the moſt part, he could only breathe in a ſitting poſture.

When the pericardium was the ſeat of the water, the patient felt generally ſtitches and ſhooting pains, with frequent palpitations of the heart, for a conſiderable time before any ſwelling of the legs, or any other complaint ſeized him. The ſwelling of the legs was not often conſiderable, the urine was ſeldom ſcanty, neither was the appetite impaired, nor the belly coſtive; and, till the laſt ſtage of the diſeaſe, the difficulty of breathing was inconſiderable. Upon a careful examination, the ribs on the left of the ſcrobiculus cordis appear more prominent than uſual; a great tenderneſs, and often an acute pain, is felt in the left ſhoulder under the clavicle, and at the pit of the ſtomach. By preſſing on that ſpot, a crackling noiſe, reſembling that in emphyſematous caſes, is alſo felt. I know not the cauſe of this, but have obſerved it only in the worſt caſes; and then alſo the heart beats ſo ſtrong and hard that the patient becomes very uneaſy, and the leaſt motion produces ſuch palpitations, faintneſs, and irregularity of the pulſe, that immediate death is threatened. He cannot lie on his back a moment, and not with eaſe on his right ſide; but can lie tolerably well, even with his head low, on the left ſide.

When this diſeaſe firſt made its appearance in 1782, I had recourſe to frequent emetics and purgatives, but not of the draſtic kind, and found, after a conſiderable trial, that I gained no ground. I therefore reſolved on trying the elaterium, and made it up into pills with extract of gentian, each pill containing a quarter of a grain of elaterium. I began by ordering one of theſe to be taken every hour till they operated; but finding they had often more violent effects than I intended, I ordered them at laſt only every two hours, till they had the deſired effect. And though ſometimes a vomiting, always a nauſea, and often a griping, was occaſioned by them, yet ſuch quantities of water were always diſcharged, both by ſtool and urine, and with ſuch relief to the patients, that I could hardly prevail on many of them to take any other medicines on the intermediate days. Finding ſucceſs from this practice, I repeated the pills every third or fourth day, till all the ſwelling was gone, and then recourſe was had to the bark and elixir of vitriol, or the bitter infuſion, in which a dram, or a dram and a half, of ſalt of tartar was diſſolved daily; and, by this means, few relapſed.

On the days I omitted the elaterium, ſquill draughts, the bitter infuſion, as above, or a dram of the ſweet ſpirit of nitre, with as much antimonial wine as could be taken without producing a nauſea, were given twice or thrice a-day with great advantage. With ſome few, however, ſmall doſes of camphire and nitre anſwered better as a diuretic than the above. Where the ſwelling extended over all the body, ſcarifications and punctures were of great uſe; and in caſes where it was not ſo conſiderable, frequently rubbing the legs with warm camphorated oil, had good effects.

Theſe were the only medicines neceſſary the firſt ſeaſon; but the next, I was under the neceſſity of varying them, for the organs of reſpiration being more particularly affected, and the anaſarcous ſwelling leſs, I was afraid to uſe the elaterium with the ſame freedom, after the above ſymptoms came on to any great degree; and beſides, I obſerved, that their ſtrength failed more ſuddenly this ſeaſon, ſo that I truſted chiefly to the ſpirit of nitre and antimonial wine, in as large doſes as they could bear them, keeping the body, and particularly the legs, as warm as poſſible, and rubbing them frequently with the warm camphorated oil. By this means, and by allowing them plenty of wine to ſupport their ſtrength, many of them recovered: the medicines keeping their bellies open, and operating powerfully as diuretics, and ſometimes even as diaphoretics.

Though I found this anſwer beſt in moſt caſes, yet, where I had obſerved the breathing to be affected from water lodged in the cellular ſubſtance of the lungs, and that an expectoration was coming on, I endeavoured to promote it, by giving ſquill and ammoniac medicines, and by drawing off as much ſerum as poſſible by bliſters applied both to the ſternum and ancles, and often with ſucceſs. I attempted, at firſt, to let out the water by punctures in the legs and feet, but found it would not anſwer this ſeaſon; the punctures inflamed to ſuch a degree, that I had much trouble in preventing a mortification from taking place; and felt myſelf very happy at getting the ſores healed again. When the complaint proved very obſtinate, I found it neceſſary to change the kind of diuretics which I uſed; for after a certain time, they loſt conſiderably of their effects, ſo that I uſed the bitter infuſion with the alkaline ſalt, the antimonial wine and ſpirit of ſalt, the ſquill preparations, the nitre and camphire, or nitre, ſquills, and pepper made into a fine powder, promiſcuouſly. And when the belly was coſtive, a dram, or a dram and a half of compound powder of jalap, anſwered as a purge. As ſoon as the oppreſſion was gone off, and the ſwelling of the legs had decreaſed, the bark, with elixir of vitriol, were had recourſe to. But I am ſorry to ſay, it had not always the deſired effects; for five or ſix (out of thirty whom I diſcharged well) had relapſes, and one of them is ſince dead.

Bleeding, in caſes where the breathing has been much obſtructed, was tried by ſeveral gentlemen, but with no good effects. Mercury has been given in this, as well as moſt other complaints here, but with bad ſucceſs. Nor were the milder purgatives of any greater uſe, though conſtantly preſcribed by many of the faculty. From obſerving that no officers were troubled with this complaint, and that the Europeans who had taken much mercury for venereal complaints, and thoſe who were moſt addicted to hard drinking ſuffered moſt by it; I never tried the uſe of calomel in any caſe. I made them begin the uſe of bark, and other ſtrengtheners as ſoon as poſſible, and allowed them to uſe no warm drinks, as tea, *&c.* but mixed ſome good gin or brandy, with their rice water. By this means, they made more urine, and their ſtomachs were not ſo much weakened by the uſe of medicine. I am convinced the ſaline and milder purgatives weaken the ſtomach and bowels much, and are rather promoters of a dropſical diſpoſition; for this reaſon I have not uſed them lately, but truſted entirely to diuretics and diaphoretics, in caſes where the bowels and ſtomach were too irritable to bear the draſtic kind. And, if the belly was at any time coſtive, ſtimulating glyſters were occaſionally given. By one or other of the above methods I treated all my patients, both ſeaſons; and with greater ſucceſs than any other practitioner that I know of here. Three, in all, died, whoſe caſes I ſubjoin; and five more have relapſed in February, and are not well yet. At that time I was indiſpoſed, and obliged to leave the army, ſo that I do not know how they were treated.

Case I. Penny (a ſtrong looking man, aged thirty) was ſeized with anaſarcous ſwellings in November 1782, and put upon a courſe of calomel by the ſurgeon who attended. In leſs than three weeks he was in a ſalivation: but the ſwelling extended over all his body. His breathing was much affected, his reſt diſturbed with horrid dreams and ſtartings. His thirſt great, urine very ſcanty, belly coſtive, ſkin cold and clammy, a ſhort dry cough diſtreſſed him much, and he could only ſleep on his right ſide, which was much more ſwelled than the other. He was eaſieſt when his head was lower than any other part of his body. In this ſituation he was brought to me. I ordered a ſtimulating glyſter immediately, and afterwards half a drachm of reſin of jalap diſſolved in ſome brandy. This purged him four or five times, and he felt himſelf eaſier that evening. The ſecond day a pint of chamomile tea, a drachm and a half of ſalt of tartar, and four ounces of gin, were mixed, and a glaſsful given every two hours. This brought on a free diſcharge of urine; but the whole body being prodigiouſly ſwelled, I made ſcarifications about the breaſt, thighs, and legs, in hopes that his breathing might be relieved by the great diſcharge. It had not the deſired effect, however. On the third day, the glyſter was repeated, the chamomile tea continued, and a large bliſter applied to the breaſt, which gave conſiderable relief for a few hours. On the fourth day, finding the ſwelling ſtill very great about the breaſt, I made freſh ſcarifications, and determined on performing the operation of the paracenteſis as ſoon as I could, if the breathing was not relieved that night by the diſcharge from the cellular membrane: for I could not do it before that time, as the ſwelling over the ribs was at leaſt ſix inches in thickneſs. His ſtrength was tolerably well ſupported by the quantity of wine he drank; and the diſcharge of urine was copious that day. But unfortunately, through the night, in going to make water, he ſtumbled on the threſhold of the door, fell down, and immediately expired. I was inſtantly called; drew off about two quarts of water from the right cavity of the thorax; attempted, by every ſtimulant, to recover him, but all in vain. There was no water in the left cavity; and I was not permitted to open the body. Probably no operation could have ſaved him; but I regretted that I could not make the trial.

Case II. John Briggs (a man of a weakly conſtitution, aged thirty-ſix, who had been poxed repeatedly ſome months before) was ſeized with the anaſarca in November 1782; but, by the uſe of the above medicines, he got well, and did duty in the field till April 1783, when he applied to me for the cure of a complaint in his breaſt, which he ſaid he felt coming on for a fortnight before. From obſerving the pale countenance, ſwelling of his ancles, (though trifling), and finding his breathing much affected when he walked, or attempted to lie down, and that his belly was very coſtive, his urine high coloured and ſcanty, and thirſt conſiderable, I had no doubt of his having water in the thorax. And, from the ſmallneſs and quickneſs of his pulſe, and the troubleſome cough which he had, I concluded the water was in the ſubſtance of the lungs.

A ſtimulating glyſter was immediately ordered, and afterwards a drachm of the compound powder of jalap; his legs ordered to be bathed in warm water, and afterwards well rubbed and kept in flannels. He felt himſelf pretty eaſy, till the next day at noon his ancles were more ſwelled, and I expected, as an expectoration of a white fluid was coming on, that his breaſt would be relieved. Squill draughts, with ammoniac, were given; his belly opened by a glyſter at night; and wine allowed with his drink. He was very reſtleſs, however, in the night, and could not lie down. The expectoration increaſed, and the oppreſſion alſo; I now applied one large bliſter to the ſternum, (for under it he ſaid he felt a pain, and a load of an immenſe weight, which he could not remove), and two to the ancles. But he diſliked them, and pulled them off before they had any effect. He turned more reſtleſs towards night; his pulſe could hardly be felt; his thirſt increaſed; a vomiting came on, and a hiccough, both of which continued for about twelve hours, till he expired.

During all that time, he expectorated a vaſt quantity of frothy white fluid, which pouring into the bronchiæ faſter than he could bring it up, ſuffocated him. On opening the body, the abdominal viſcera were ſound; but when I raiſed the ſternum, the lungs burſted out over the ribs, and appeared ſo large, that I could hardly think the whole thorax ſufficient to contain one lobe of them. On preſſing them, a torrent of the white frothy fluid guſhed out at the mouth; and, on cutting, a quantity of air and water bubbled out, and then they collapſed. There was no water any where elſe. I opened two black men afterwards, who died of the ſame complaint. They were the patients of another gentleman. The appearances were nearly the ſame.

Case III. John Kitchen (a ſtrong robuſt man, aged 32, much addicted to drinking) complained to me, in October laſt, that, upon walking much, he had a pain and ſtrong palpitations at his heart. I ordered ſome cooling phyſic, adviſed reſt, keeping out of the rain, and avoiding ſpirituous liquors. I did not ſee him any more for a month, when he complained of ſome ſwelling, tightneſs, and weakneſs of his legs; pain at the pit of the ſtomach, and under the left clavicle; and frequent ſhooting pains about the heart. His appetite was good, his belly open, and his colour natural, but urine ſcanty. A drachm of ſweet ſpirit of nitre, and forty drops of antimonial wine, were given three times a-day; a light nouriſhing diet preſcribed; and the body and legs kept in flannels and worſted ſtockings. In the courſe of three weeks, the complaints went off; the palpitations leſſened; and the bark, with elixir of vitriol, entirely removed it.

About five weeks after he was diſcharged, he returned to the hoſpital with the ſame complaint. I was not then with the army, and know not what he took at firſt; but his difficulty of breathing increaſed ſo much, that the ſurgeon who attended bled him, two days before he died, which gave no relief. The next day he gave him tartar emetic, which vomited him, and made him worſe. That evening I went to camp, and found him in the greateſt agony; his breathing almoſt ſtopped; his pulſe weak and intermitting; his heart beating moſt aſtoniſhingly ſtrong and irregular; the epigaſtric region ſwelled; the thirſt unquenchable; the vomiting inceſſant; and the pain at ſtomach excruciating. I ordered a warm glyſter, fomentations to the pit of the ſtomach, a bliſter to be applied to the breaſt, and ſinapiſms to the feet: but he expired before they were ready.

The next morning I opened him; found the liver and ſpleen greatly enlarged, and very tender in their ſubſtance, and ſo diſtended with blood that hardly any was to be found in any other part of the body, excepting the heart. The ſtomach appeared a little inflamed; but there was no water, or any other morbid appearance, in the abdomen. On raiſing the ſternum, I found that I cut away a large portion of the pericardium, which adhered to it, and that about a quart of water guſhed out, and nearly the ſame quantity ſtill remained. The heart was double its natural ſize, diſtended with blood; and the pericardium adhered ſo firmly to the ſternum and pleura before, and had ſqueezed the mediaſtinum and lungs ſo cloſely to the ſides and back part of the thorax, that nothing but one large cavity, containing the heart, floating (as it were) in water, could be ſeen. The lungs were formed into two thin cakes, adhering firmly to the pericardium before, and to the pleura behind, of a greyiſh colour, hardly containing any blood, air, or water. There was a quantity of coagulated blood in the heart; but I do not think it had formed into a polypus before death.

Theſe were the patients that died under my care. I opened a number of black people, where I found the pericardium diſtended, containing a pint, or more, water; and, in ſome, the lungs adhered very firmly to the pleura. But in none were the parts ſo diſeaſed as in the above. And, in many, the vaſt enlargement of the liver itſelf, though accompanied with ſome ſwelling, ſeemed to occaſion death; at leaſt, the quantities of water in the thorax were not ſufficient to produce ſuch effects alone.

II.

Some Remarks on certain Articles of the Materia Medica, communicated in a Letter to Dr Duncan, *from Antigua, by Dr* James Adair, *now Phyſician in Wincheſter.*

IN addition to the hints which I formerly communicated to you, reſpecting different articles of the materia medica, I now ſend you the following obſervations.

Alumen Rupeum. During the autumnal and winter months of 1779, the intermittents in this iſland had more of an aguiſh aſpect than I ever remarked before in this country; and ſome of them were real quartans. The bark given alone rarely cured them in my practice; but an addition of from five to ſeven and a half grains of alum, and as much canella alba, to one drachm of bark, and repeated three, four, or five times a-day, removed the fever, and prevented relapſe. I uſed the alum in one caſe of colica pictonum, with advantage: it was given from ten to fifteen grains for a doſe, with as much ſpermaceti. Joined with ſpermaceti and opium, it is an uſeful and ſafe aſtringent in hectic diarrhœa.

ACETUM LITHARGYRITES. As this medicine is unequal in point of ſtrength, according as the vinegar is more or leſs pure, I tried the vitriolic acid diluted, in place of the vinegar; but it does not ſeem to be ſo complete a ſolvent of lead as good vinegar. But the ſaccharum ſaturni is a much more certain preparation than either.

RAD. IPECACUANHÆ. The emetic tartar and calomel, mentioned as a febrifuge in my former remarks, having ſometimes operated with more violence, in certain caſes and ſtages of fever, than I could have wiſhed, I have now ſubſtituted the ipecacuanha in place of the emetic tartar; and, aided by the calomel, it is preferable in children and delicate perſons. The proportion is, three parts of calomel to two of ipecacuanha.

Sal Catharticum Amarum. In my former letter, I mentioned ſome means which I had uſed in the epidemic dyſentery; but in the advance of the epidemic, it became more obſtinate, and I was obliged to have recourſe to the following, with much more ſucceſs. By the following, the ſtools became leſs frequent and bloody, and more copious, whilſt the tormina were, at the ſame time, much alleviated. ℞. Sal. cathart. ſex uncias. Solve in aq. font. ℔iſs.; et add. acid. vitriolic. q. ſ. ad gratam acerbitatem. Of this four ſpoonfuls for an adult were given three or four times a-day, for two or three days without intermiſſion. In ſome recent caſes, it was the only medicine I had occaſion to uſe; and I did not, until the evening of the ſecond or third day, give any opiate; and then I gave either Dover's powder, or the pill formerly taken notice of, a compound of vitrum antimonii, calomel and opium. Having uſed the ſalts for two, three, or four days, I ſubſtituted, in many caſes, the vitrum antimonii ceratum; and, after expending all the vitrum antimonii I had, I cerated the crocus metallorum, which ſeemed to be equally effectual in a larger doſe. This medicine became neceſſary in more inveterate caſes; ſometimes combined with calomel, and an opiate at night, if the patient was much diſturbed.

FIXED AIR. Nauſea and vomiting is one of the moſt perplexing, and indeed dangerous, ſymptoms that occurs in our fevers, eſpecially when it continues through the remiſſions; as it puts it out of our power to uſe the neceſſary means for removing the paroxyſm, or preventing a return. When other means fail, the magneſia or chalk waſhed down by diluted vegetable or vitriolic acid, have checked it. Were the fixed alkaline ſalt leſs unpalatable, it would be preferable. This delicacy of ſtomach is often complicated with a ſenſe of weight and fulneſs of the belly, and ſuch patients are purged with difficulty. This ſpecies of remittent, reſembling the *febris meſenterica* of Hoffman, is very untoward; the patient is never eaſy, unleſs when under the operation of a purgative; and the bark is either unavailing, or exaſperates the complaints. It ſeems to proceed from ſpaſm as a cauſe, and congeſtion as an effect; hence morbid determination, ſimilar to what produces febrile headach, delirium, dyſpnœa, anxiety, *&c.* Here alſo magneſia, with ſome grains of jalap, the diluted acids, and emollient glyſters, are the beſt means of relief. Let me add, that notwithſtanding De Haen's ſtricture, I am convinced that Storck's febris hæmorrhoidalis does ſometimes occur, of which I have ſeen three attacks in an old corpulent Iriſhman. A ſudden and ſevere rigour was followed almoſt inſtantly by a ſtupor; and, if the hæmorrhoidal veſſels had not burſt, and diſcharged copiouſly, he muſt have been deſtroyed.

In no inſtance have I ſeen better effects from the fixed air than in an incipient phthiſis pulmonalis. It mitigates, and ſometimes entirely takes off the hectic fever. I give one drachm of powder of chalk, and one grain or two of ipecacuanha, three or four times a-day, and waſh it down with the acid. I generally aſſiſt it with the extractum cicutæ, adding ſometimes one grain of calomel, interpoſing emetics, and bliſtering the back, or pit of the ſtomach. I have not yet experienced any remarkable benefit from the fixed air received into the lungs.

VITRIOLUM CÆRULEUM. Having tried the bark, without ſucceſs, in ſome obſtinate agues, I ſubſtituted the following with advantage: ℞. Canell. alb. noſtr. unciam unam, vitriol. cærul. grana ſex. Syr. q. ſ. f. pilul. No. 80. Doſe from three to ſix, three or four times in the intermiſſion. But this has failed in the caſe of a quartan in my own family, though I afterwards added alum and Cayenne pepper, vomited ſometimes at the acceſſion, ſometimes gave large doſes of volatile alkali, with opium, to raiſe a ſweat, and anticipate the acceſs; but was equally unſucceſsful. This obſtinate ague was at length removed by the ſal ammoniac. crud. from one drachm to four ſcruples thrice a-day; but, as it purged during the firſt days, I added a little laudanum to prevent this effect.

Hæmorrhages are very frequent in the negroes in this iſland; at leaſt many caſes have occurred to me. The moſt frequent ſpecies are the hæmaturia, and what ſome authors have called *fluxus hepaticus*, being a liquid bloody diſcharge without fever, or *tormina* to any degree;

but which the patients and nurſes never fail to conſider as flux. This hæmorrhagic diatheſis may be owing partly to general laxity, partly to tenuity of the fluids. The following has been of more uſe than the bark or any other means.

℞. Vitriol. Roman. grana xii. ſolve in aq. font. ℔iſs. et add. acid. vitriolic. q. ſ. ad gratam acerbitatem. Doſe from one to three ſpoonfuls twice or thrice a-day, ſo as to keep the belly gently open. Sometimes I have added nitre, where there was preternatural heat and quickneſs of pulſe. But this is rarely the caſe; for theſe hæmorrhages are, in general, rather paſſive than active.

Emmenagogues. In my former remarks, I obſerved, that, in two or three recent caſes, the extractum cicutæ ſeemed to reſtore the flux; but I have experienced more ſucceſs from the following courſe:

1*ſt*, A gentle mercurial purge, repeated once or twice in the courſe of a week.

2*d*, A vomit once a-week, for three weeks.

3*d*, The week after the uſe of the mercurial purges, I begin with the following:

℞. Aloes hepatic. canell. alb. noſtr. ana ℥ſs. pulv. cantharid. ℈i. miſce affund. ſpt. ſacchari et aq. font. ana ℔i.; et poſt ſeptiman. cola. Doſe from half to two table ſpoonfuls every morning, ſo as to purge gently. If the bowels are irritable, I give it in the evening with laudanum, or the opiate alone.

This medicine I have uſed in thoſe caſes only where not only the uterine, but the general ſyſtem of fibres ſeemed to be flaccid, and their irritability greater than natural; and indeed ſuch is the ſtate of the greateſt number of theſe patients; and in ſuch the periodic efforts are very obſcure, or not at all obſervable; and therefore I uſed this remedy conſtantly, without regard to periods or conſtitutional efforts. In ſome caſes, indeed, where the diſcharge has been checked by cold, the ſymptoms of pain and ſpaſm have been pretty urgent; and, in ſuch caſes, vapor uterinus, warm pediluvium and opium, or extractum cicutæ, with compreſſion of the femoral veſſels, have been of uſe. But theſe caſes are here very rare.

Now I am upon the internal uſe of cantharides; you will ſuppoſe that analogy has led me to uſe them in other caſes. I have uſed them, generally joined with warm ſtimulants, in cachexy, aſphyxia cachectica, (a fatal ſpecies of deliquium, which carried off many low negroes during the autumns of the laſt three years), fluor albus, diabetes, hæmaturia paſſiva, palſy, and dropſy. It is alſo a good and ſafe cordial in hyſteric caſes, which are here rarely accompanied with plethora: and I intend to extend its uſe to obſtinate cutaneous caſes, and epilepſy.

I very often uſe the tincture; but in any form, even the ſubſtance, I have remarked no inconvenience from cantharides, except gripings, and purging ſometimes, which have ceaſed either by deſiſting for a day or two, or giving an opiate.

III.

The Hiſtory of a Caſe of Catalepſis, ſucceſsfully treated by Dr J. Fitzpatrick *of Dublin.*

THE patient whoſe caſe I am to deſcribe, Mrs G—— of P——, was a full woman, about the age of thirty-five. She was the mother of ſeveral children; but, at the time of her illneſs, was not pregnant. She had not been ſo well for ſome time before with reſpect to the quantity of her menſtrual diſcharge as formerly, as it had gradually diminiſhed for ſome periods. But in other reſpects ſhe was in perfect health.

In this ſituation, as ſhe was ſtanding at a table, with ſome clothes in her hand, ſhe ſuddenly, without making any ſort of complaint to a maid then in the room, inclined forward, and remained in that poſition for ſome time. The maid's curioſity induced her to ſtir her miſtreſs, on which ſhe ſeemed to fall, or incline ſtill more over the table, but did not, in any reſpect,

ſpect, reſiſt the maid. This ſurpriſed the maid very much, and finding her miſtreſs both ſpeechleſs and motionleſs, ſhe called for the aſſiſtance of the other ſervants, who endeavoured to rouſe her by the vapours of hartſhorn, vinegar, and the like, but to no purpoſe. She was then laid on a bed; a vein was opened, but not a drop of blood was diſcharged. In vain did they endeavour to awake her; and, after ſpending near two hours in a variety of treatment, they laid her out for dead with all the uſual parade, ſuch as lighting candles, tying her feet together, ſtretching her limbs, and the like.

I was near ſix miles from her habitation when ſhe was firſt ſeized in the manner deſcribed. And ſeveral hours had elapſed from the time of the attack before I ſaw her. Upon entering the room, I was told by a clergyman, that all was over, for that ſhe had been dead for more than two hours. Hearing, however, of the manner in which ſhe was found by the maid, I was anxious to make every neceſſary enquiry into her ſituation. And, after having aſked ſeveral queſtions with reſpect to the attitude in which ſhe was firſt diſcovered, I concluded

ded that the original attack was cataleptic. I therefore ordered a number of people out of the room, and merely permitted ſuch to remain as might be uſeful.

The firſt thing which I thought neceſſary was to raiſe her hands, and having found them in ſuch a ſtate as to remain in any poſition in which I placed them, I had no longer the leaſt doubt of the exiſtence of a real catalepſia. I inſtantly looſed her legs, and every other ſtricture which had been placed about her chin and breaſt. I removed the cold ſheets, and ordered her to be wrapped in warm blankets. I directed that two girdles ſhould be warmed, one of which was to be introduced under the ſhoulders and back, and the other lower down. Hot bricks were likewiſe applied to the ſoles of her feet; and her body, legs, and arms were rubbed with very ſtrong ſpirits and muſtard. I applied eau de luce, and ſtrong hartſhorn to the noſe. I introduced volatile ſpirits as far down the throat as I could by means of a female catheter. But all theſe meaſures, though frequently repeated, were to little purpoſe.

Having tried theſe active remedies, together with the other agents mentioned, and not being able to diſcover either pulſation or breathing, excepting, as I imagined, a ſlight tremulous motion of the heart, my hopes were almoſt at an end. But I was determined to perſiſt, in order, if poſſible, to relieve her. To effect this, an injection was prepared of garlic, tobacco, ſalt, and urine. And this was ſeconded by conveying ſome oil of vitriol and aquafortis into her noſe by means of a feather laid gently on each ſide of the ſeptum, or middle cartilage, and introduced for more than an inch upwards. On this application ſhe ſeemed to wrinch. This induced me to perſiſt in the frictions and ſtimulant treatment. And, after perſeverance for an hour, ſhe ſhewed evident ſigns of being affected. In leſs than two hours ſhe opened her eyes. She attempted alſo to ſpeak, but could not articulate. She recovered, however, in ſome degree the power of ſwallowing, and I now gave her, from time to time, a little white wine, with a few drops of hartſhorn. I repeated the injection, which, after a few minutes, had now the wiſhed for effect.

From the firſt viſible appearance of life, I took care to remove from the room every thing which could lead her, in the leaſt degree, to underſtand the dangerous ſituation in which ſhe was, leſt this circumſtance coming to her knowledge might be attended with bad conſequences. Her recovery went on gradually, and after it was completed, ſhe told me that, for ſome time before I came, ſhe had a knowledge of what the attendants were doing about her. But ſhe had not, in the ſmalleſt degree, the power of uſing any joint of her body, of ſpeaking a ſingle word, or of opening her eyes. When they were accidentally opened, ſhe could not ſee; ſo that ſhe might be conſidered as completely paſſive.

A few days after her recovery from this attack, I put her on a deobſtruent courſe, with a view to the menſtrual diſcharge. This had a good effect. Since her recovery ſhe has had two children, and remains in tolerable health.

IV.

Account of the good Effects obtained from the Calx of Zinc, in a hyſterical Affection. Communicated to Dr Duncan, *in a Letter from Glaſgow, by Dr* Alexander Maclachlan.

M——R, aged nine years, of a ſprightly diſpoſition, is ſeveral times a-day attacked with a conſiderable pain through the abdomen, particularly ſevere about the umbilical region. When this has continued for ſome ſpace of time, it is ſucceeded by loud borborygmi, and the feeling as if of a heavy ball aſcending towards the throat, producing a ſenſe of ſuffocation. She then falls down motionleſs, with the loſs of all her ſenſes, hearing excepted. And after ſhe has remained in this ſtate for about ten minutes, with a twitching of the extremities, ſhe gradually recovers.

During the paroxyſm, ſhe has alſo a circumſcribed pain in the upper part of the forehead, which ſhe reſembles to that of a nail driven in by a hammer; and after the fit has ceaſed, a cold clammy ſweat ſupervenes, which after a ſhort time diſappears. Tongue and belly natural, pulſe 100, and ſomewhat hard.

Theſe complaints have been but of ſhort duration; the fits ſhe has only had for four days, during which, they have attacked her ten or a dozen times; but the affecton of the abdomen and head are of ſome weeks ſtanding. She has uſed no medicines, but a bolus of rhubarb and calomel adminiſtered to her, upon a ſuſpicion of worms. I ſaw her upon the 27th January, when the ſymptoms were as above; I immediately directed eight ounces of blood to be taken from her arm, to have the warm pediluvium for an hour in the evening, and immediately thereafter an anodyne draught.

28th, I found her pulſe at ninety, and ſofter; but ſhe had had but an indifferent night, and three fits ſince the veneſection. Upon which, with a view to give a fair trial to the calx of zinc, I ordered the following:

℞ Calc. zinc. extract. gentian. craſſior. utriuſque grana viginti et quatuor; ſapon. Hiſpan. ſemidrachmam. Syr. ſimpl. q. ſ. fiat maſſa, divid. in pil. 24. Cap. pil. duas omni man. et veſpere.

39th, Free from fits; but ſtill complains of headach and pain in the abdomen.

2d February, Has continued the pills ſince laſt report; and ſhe not only remains free from the fits, but has neither headach, nor pain in the abdomen.

On the 5th, She continued free from any ſymptoms, I then ordered the pills to be intermitted, and ſhe has ſince continued in good health.

V.

Account of an uncommon Caſe in Midwifery, where a preternatural Adheſion of Twins had taken Place. By Dr James Oliphant, *Phyſician at Irvine, communicated to Dr* Monro.

ON Tueſday, November 9. 1784, (about two P. M.) the wife of W. C. near Irvine, was ſeized with her labour, attended at firſt with a conſiderable flooding; but that diſcharge abated as the pains increaſed. At five the head and arms of a dead child were born; but the midwife finding the extraction of the reſt of the body impracticable, was of opinion that the womb contained twins, whoſe bodies were grown together, and deſired I might be ſent for. I arrived at the houſe about ſix.

Strange as this report of the midwife ſeemed, upon the moſt accurate examination, I found it exactly true; for trying to finiſh the delivery by the purchaſe which the head gave me, I found ſuch a reſiſtance, as if the child were grown to, or formed a part of the mother's body.

To diſcover the cauſe of this reſiſtance, (the woman being laid on her back), I introduced my left hand below the head and breaſt of the firſt body, which had preſented in the natural poſture, with the face to the os ſacrum, and ſoon felt a firm cartilaginous ridge, that evidently connected the firſt body with another. The funus umbilicalis made two turns round the preternatural union of the two bodies. The breaſt and neck of this ſecond body ſoon conducted me to its head, which I found turned upwards, with the lower jaw reſting upon the right brim of the pelvis. This depraved poſition accounted for the great reſiſtance, and, at the ſame time, pointed out the means of relief. Puſhing the bodies upwards, and towards the left ſide, I eaſily inſinuated my fingers between the brim of the pelvis and chin of the child, and pulling downwards, till I could introduce a finger into the mouth, I ſoon ſucceeded ſo far as to get the whole of the ſecond head into the pelvis, but with the convex part of the occiput into the concavity of the os ſacrum, and the face of the child preſenting to the pubes of the mother.

As from the manner in which the two bodies were connected, this bad poſture did not ſeem to admit of any remedy, I proceeded, without regarding it, to finiſh the delivery as well as I could.

The hind part of the ſecond head remaining in the hollow of my left hand, I cauſed the midwife to pull by the firſt head, while I employed my right hand in occaſionally forwarding the ſecond head or firſt body. A ſtrong pain aſſiſting, by the joint exertion of our whole ſtrength, firſt that head came down, and then the united bodies immediately followed. And though the laſt head, the firſt ſhoulders, and both my hands, muſt have paſſed at the ſame time, the poor woman was ſafely and happily delivered in leſs than a quarter of an hour after my arrival, and has ſince had a very good recovery, and feels no hurt or bad conſequence from ſo great a dilatation of the parts.

Mrs C. is a broad well formed little woman, with a large pelvis. She is thirty-nine years old; was married young; and had before brought forth ten children, of which the firſt was born dead, and all the other nine alive, well ſhaped and healthy. Her menſes had been ſuppreſſed for ſeven weeks, but returned of their own accord; and immediately upon the diſcharge ceaſing, ſhe became pregnant of this double fœtus, which ſhe brought forth at the uſual term of nine months. During the laſt months of geſtation, ſhe was very big and unwieldy, but always able to do her family buſineſs, and, in other reſpects, healthy. She believed herſelf with twins, and perceived the whole weight to fall to the ſide on which ſhe lay in bed. The left ſide of her belly felt cold for two or three days before her delivery; and the largeſt child, whoſe head was firſt born, appeared to have been ſo many days dead. The other child was felt to ſtir a little before its birth, but never after.

The membranes that contained this double body, were natural, large, and ſtrong. The placenta was ſingle, very large, and thick; on one ſide, it was ſomewhat ſhrivelled and decayed, as if ſome time torn or ſeparated from the uterus; from the correſpondent part of which, the hæmorrhage that preceded the labour moſt probably iſſued. Near its centre, two ſeparate umbilical cords originated, at about an inch and an half diſtance from each other: the umbilical arteries and veins were complete and perfect in each. About three inches from their origin in the placenta, they were united by a fleſhy ligament, and ſo contorted during their whole courſe, as to ſeem but one, but much thicker and longer than uſual. About two inches from its other extremity, this cord again divided into two, and each furniſhed with its proper veſſels, terminated in the navel of its proper body.

The united bodies were both males, completely formed in every part, full grown, and each body as large as an ordinary ſingle child.

The heads, necks, and arms, above, and all the parts below the umbilici, as well as the whole poſterior parts of both bodies, were ſeparate and diſtinct; and every feature, limb, and member, of its due ſize, number, figure, and ſituation, in every reſpect, except what related to their preternatural union.

They adhered by their anterior parts ſo, that when placed ſidewiſe, they ſeemed to embrace each other. The bodies, though contiguous, were by no means confuſed or blended, but ſeemed like two different bodies, in ſuperficial contact with each other.

The union began at the interior and convex part of each of the clavicles; which were ſtrongly connected to each other by firm cartilages. The ſternum of each ſo exactly agreed, as if they had had but one in common to both.

The cartilages of the ribs of the left ſide of the one were ſo accurately and firmly united to the cartilages of the correſponding ribs of the right ſide of the other, that the ſulcus formed by the points of contact on both ſides, was, upon compariſon, found exactly ſimilar and equal.

Below the ribs, the bodies continued joined by the common integuments, till the preternatural coheſion terminated about an inch above the two navels.

I have no doubt but the internal parts were all double and complete in each body. I wiſhed much to have been able to aſcertain by diſſection, how far, had the bodies been born alive, an artificial ſeparation of the two, by ſurgical operation, might have ſucceeded. But the prejudice of the parents would not admit of any thing more than a ſuperficial inſpection.

VI.

Hiſtory of two Caſes of painful Conſtipation from indurated Fæces, with remarks on that Affection. By Dr John Warren, *Phyſician at Taunton, communicated in a Letter to Dr* Duncan.

AS the Medical Commentaries will, under your direction, continue to furniſh valuable, cheap, and extenſive information to the practitioners of medicine; in addition to the tenth article of the fourth volume of Medical Obſervations and Inquires, London *, I am deſirous of communicating to you ſome farther account of the ſame diſeaſe. It has frequently occurred to me in practice, and has ſeldom been taken notice of by any author; the cauſe ſeems to be little known to the Faculty in general, and when miſunderſtood, is often attended with fatal conſequences.

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* On painful conſtipation from indurated fæces.

The malady I allude to, aſſumes the appearance of a diarrhœa, but is, in fact, a conſtipation, or, at leaſt, owes its origin to an unuſual and conſiderable collection of indurated fæces in the rectum.

The two following caſes will, in ſome degree, ſerve to illuſtrate the hiſtory of this diſeaſe, and will tend, in a great meaſure, to particularize it.

I was ſome years ago, requeſted to viſit an elderly gentleman, who had been ill more than twelve months of an obſtinate diarrhœa, according to the report of his friends and apothecary. I found that during this period, he was reduced from firm health and ſtrength, to a very great ſtate of weakneſs; his pulſe ſmall and quick. He complained of inceſſant thirſt, and of frequent and ſevere griping pains in the lower region of his belly, accompanied with perpetual bearing down, and almoſt continual inclination to evacuate the contents of the inteſtines. The diſcharge, however, was, in general, trifling, always lax, and frequently mixed with ſmall knobs of hardened excrement, not much diſſimilar to the common appearance of ſheeps dung. He had taken, during this long and tedious indiſpoſition, rhubarb, ſeparately and conjointly with aſtringents, boles, opiates, and, in ſhort, every ſpecies of medicine, commonly preſcribed in caſes of diarrhœa, without reaping from them the leaſt advantage. It was from laudanum, alone, that he had found any temporary benefit, of which he was now obliged to take large and frequently repeated doſes. I was ſoon convinced from the concurrence of theſe and other circumſtances, that this caſe originated from ſtercoraceous matter lodged in the rectum, and propoſed to the patient that his ſurgeon ſhould examine him, by introducing into the anus his finger, a ſmall candle, or marrow-ſpoon. But this operation he ſtrenuouſly oppoſed, from an idea of the great pain he thought it would give him, the anus being in an inflamed excoriated ſtate. Oily glyſters were alſo objected to for the ſame reaſon. Thus, he lingered for ſome weeks, and at length expired. The day before his death, having relented of his obſtinacy, he readily conſented to have the operation performed, which would, at firſt, have been alone capable of affording him laſting relief; when, to his great aſtoniſhment, ſome pounds of indurated fæces were ſcooped out of the rectum, by means of a marrow-ſpoon; and much more could have been extracted, had not a frequent recurrence of ſyncope rendered it dangerous to proceed.

In the other caſe, I was more fortunate. A lady, ſixty-four years of age, had laboured five months, prior to my ſeeing her, under what was ſuppoſed to be a conſtant purging, attended, from the commencement of the complaint, with more or leſs teneſmus. The pains in the lower region of the abdomen, were not conſtant, but recurred every hour or two immediately preceding each effort to exonerate the inteſtinal canal, during which they were ſo excruciating, as to be almoſt intolerable. A few minutes after the evacuation, (which was, in general, inconſiderable, frequently bloody, and often of that button-like appearance noticed in the other caſe), ſhe became eaſy, and continued ſo to the next period, when the ſame degree of torture was again to be endured. The pains ſhe felt at theſe times ſhe could only compare to the excruciating ones attendant on delivery. She had now a conſtant fever, was greatly emaciated, and had a general deſpondency about her. This lady, who had been attended by ſome ingenious gentlemen of the Faculty, had alſo gone through the whole routine of practice, commonly adopted in diarrhœa, with as little advantage as was experienced in the former caſe. I communicated to her my idea of her complaints, and ſhe readily aſſented to the operation I hinted at above. I cannot better expreſs the ſucceſs of it, than by copying the letter which I received from her apothecary ſome few weeks afterwards.

“ The evening you left us, I extracted from “ our patient four large balls of hardened ex-“ crementitious matter, about the ſize of hens “ eggs. The next morning ſhe diſcharged “ near twenty, and has continued to paſs more “ or leſs of them every day until about a week “ ago, when ſhe took a larger doſe than ordi-“ nary of caſtor oil, which evacuated eighteen

“ lumps of the ſame ſize as formerly. Since
“ that her ſtools have been natural, none of
“ the balls having appeared. Her medicines
“ agree well with her, and her ſtrength has
“ been gradually mending from the time you
“ ſaw her.”

That this diſeaſe may, in future, be more eaſily diſtinguiſhed from diarrhœa, which is ſo oppoſite to it in its nature, and with which it is ſo apt to be confounded, it may not be improper to remark, that in each of the caſes above narrated, as well as in every other which I have ſeen of this diſorder, the pain is principally ſeated in the lower part of the abdomen, and is always accompanied with a teneſmus, or a ſenſe of bearing down, which is never attendant on a ſimple diarrhœa; that the motions are at all times inconſiderable, and in general mixed with ſmall ſcybala; and that it is a diſeaſe which does not yield to any remedy adminiſtered under the ſuppoſition of its being only a purging. It is likewiſe worthy of notice, as a farther diagnoſtic, that the pains attending it are infinitely more acute, than any ever experienced in a diarrhœa. I muſt alſo add, that it is a diſorder peculiar to perſons in advanced life, (pregnant women excepted), as I have never met with an inſtance of it in patients of any age under ſixty. I have, moreover, obſerved, that the female ſex are more obnoxious to it than the male; which is owing, perhaps, to their coſtive habit, induced by the ſedentary lives they generally lead. Indeed, whatever tends to leſſen the periſtaltic motion of the bowels, muſt afford prediſpoſition to it; and it is known to have been frequently occaſioned by a long continued uſe of bark, opiates, and the like. A paralytic affection of the inteſtines, which is by no means an unuſual complaint in advanced life, muſt likewiſe prove favourable to ſuch a lodgement; and I have often known it to ariſe from this cauſe.

Having ſaid thus much of the diſeaſe, of the means of diſtinguiſhing it, and of its cauſes, permit me, ſhortly, to offer a few conjectures, explanatory of the ſymptoms.

It is readily to be conceived, that any unuſual quantity of fæces cannot long remain impacted in the rectum, without occaſioning uneaſineſs, and a natural effort to diſcharge them. How much, then, will not ſuch ſenſations be increaſed, when by long remora of a large accumulation, the parts become diſtended to an enormous degree? may not the whole train of haraſſing ſymptoms above deſcribed, be eaſily accounted for, from irritation, generally, independent of inflammation? With regard to the excruciating pains, and liquid operations, blended with ſmall and hard knobs, I am of opinion that one cauſe is adequate to produce both effects. The figured portions of theſe diſcharges appear to be primarily formed in the ileum, by an unuſual diſtenſion there; becauſe I do not believe they could have been thus moulded by any other part, ſo exactly to reſemble the fæces of ſheep, in ſhape, ſize, and conſiſtence. Theſe, in their paſſage downwards, muſt carry with them the mucus of the villous coat; and, when Nature endeavours to diſencumber herſelf, the periſtaltic motion ſeems much accelerated by her efforts, and the irritation thence ariſing: and could ſhe now diſembogue copiouſly and feculently, our patients would feel infinite eaſe and ſatisfaction; but, unfortunately, ſhe meets with an obſtacle not far diſtant from the anus, inſuperable to her. There this increaſed periſtaltic motion muſt terminate, and gives teneſmus. An atonic ſtate of the bowels brought on by old age, or whatever elſe that produces the alvus tarda et debilis, favours a great accumulation and long retention in mancipio recti, where the greateſt and very excruciating pains are felt. Every attempt to evacuate, will ſuperadd an aggravating cauſe, from the protruded fæces of the ileum, *&c.* being added to, and ultimately detained by the collected maſs in the rectum, already loaded, and ſometimes inflamed. Here, then, conſiderable irritation takes place, and a continuance of immenſe pain will be kept up, until this volume of congeſted matter be totally removed, and the parts recover a proper tone regularly to perform their offices. In general, the moſt fluid contents of the viſcera, and a flow of mucus, proceeding from the irritated rectum, are puſhed forwards by torturing throes, beyond the obſtructing cauſe; but we are taught by experience, to expect relief only in proportion to the expulſion of the aggregate of collective ſordes of the primæ viæ, the fluid diſcharges, tending ſolely ſtill more to incommode and haraſs our patients.

With reſpect to the cure, the plan of treatment which I have found the moſt ſucceſsful, is to diſlodge, by manual operation, as much of the contents of the rectum as poſſible, and, with a marrow-ſpoon, to break down the texture of thoſe remaining, in order to facilitate their after diſcharge. Sometimes the accumulation is found to take place beyond the reach of a finger or marrow ſpoon; in ſuch caſes, I would adviſe the introduction of a ſmall tallow-candle, as practiſed by the author in the Obſervations and Inquiries above alluded to. Oily glyſters are then to be thrown up frequently; and ſmall doſes of oleum ricini are from time to time to be exhibited. When, by theſe means, the rectum is unloaded, the tonic plan is to be purſued; and though the Peruvian bark might, in this view, be deemed (*a priori*) an excellent remedy, yet I have conſtantly found it to be prejudicial, from its tendency to induce coſtiveneſs, and therefore favourable to the cauſe of this diſeaſe. The flores martiales, joined with the extractum gentianæ, taken twice or thrice daily, and waſhed down with a pretty large draught of alum whey, I have frequently experienced of infinite ſervice. But conſtant attention muſt be paid to the daily alvine evacuations; and the oleum ricini, with oily glyſters, are, in this ſtage of the diſeaſe, and with this view, the moſt proper means of effecting that purpoſe.

In recent and ſlight caſes, I have more than once ſeen excellent effects from a briſk cathartic, which has immediately thrown off the malady, and produced a cure without any other means whatever being employed, although draſtic purgatives are moſt commonly deleterious.

I ſhall feel very ſenſible pleaſure, if theſe hints be productive of any good conſequences to ſociety.

VII.

Obſervations on the Peruvian Bark. By T. Collingwood, *Surgeon, Alnwick.*

THE grandeſt and moſt valuable acquiſitions in medicine and ſurgery have been diſcovered by accident, and often by the moſt illiterate and unobſerving part of mankind. The Peruvian bark (the ſubject of the ſubſequent obſervations) was found to be beneficial and ſalutary to man by the following accident, if oral tradition can be depended on. An earthquake happening in the neighbourhood of Loxa, ſo ſhook the banks of a lake that the trees thereon fell into the water, and impregnated it with healing qualities. A native, ill of an intermittent, who had tried all remedies in vain, had recourſe to the waters of the above lake, more to quench his thirſt than expecting any medical virtues therefrom. But finding himſelf daily gain ſtrength, he perſevered in this remedy till perfectly recovered.

This unexpected cure being enquired into by ſome of the principal inhabitants of Loxa; the affair being inveſtigated, produced the happy diſcovery.

The natives were not long acquainted with this valuable medicine, until the fraudulent Spaniards extorted the ſecret from them in 1537, when the Jeſuits brought the bark to Europe, and ſold it at the enormous price of L. 30 *per* pound weight.

The celebrated botaniſt Juſſieu taught them how to diſtinguiſh the good from the middling ſorts. A friend of mine, that has reſided about ten years in Peru, favoured me with the following deſcription, which correſponds with the account given of it by Mr Arrot in the Philoſophical Tranſactions, No. 446.

The tree grows in Peru, between 2 and 5 degrees of ſouth latitude. The beſt is near the city of Loxa. It grows very tall, without branches till near the top, where it ſpreads out to a regular hemiſphere, the trunk (aſcending) tapering thicker than a man's thigh from the root upwards. The bark is of a blackiſh colour, on the outſide, having frequently whitiſh ſpots, and a kind of fungus or moſs growing upon it. The leaves reſemble thoſe of a plumb tree, of a dark green on the upper or convex ſide. The wood is as hard as our Engliſh aſh or elm, and almoſt as tough. There are four kinds of bark; the reddiſh, the yellowiſh, the curling, and the whitiſh. The two firſt are the beſt. The curled is cut from young trees or branches; and the white ſoon becomes inſipid. As the beſt ſort is become very ſcarce, great quantities of the other kinds are cut and ſent, with a little of the beſt, to Panama, for Europe.

I might lengthen out Mr Arrot's, or my friend's, deſcription of the method of drying it, and other particulars. But the curious may be ſatisfied by looking into the Philoſophical Tranſactions above mentioned.

The faculty of this iſland have been long miſtaken as to the beſt bark, generally chuſing the quill kind as the moſt valuable, while the other kinds were in uſe on the continent, and muſt have been more efficacious. An accident with which the annals of medicine are pregnant, put it lately in the power of the faculty to diſtinguiſh the reddiſh bark from the other ſorts. A Spaniſh ſhip (from Lima) captured in the courſe of laſt war, had on board a large quantity of the red bark, which was purchaſed by the druggiſts in London, and found by different experiments (pharmaceutical as well as practical) to be preferable to that quill kind imported for a great number of years into our iſland. No doubt, the valuable diſcovery of a medicine ſtill ſuperior (to that which, for many years, had been termed the grand ſpecific in intermittents, and lately found equally efficacious in almoſt all putrid and nervous diſorders), would ſtimulate the inquiſitive and benevolent to give a candid trial to this ſtill more valuable medicine.

Dr Saunders is the firſt that takes notice of its ſuperior efficacy, and many others, he mentions, have trodden the ſame path, nor have they been diſappointed. I am of that party, having ſoon after its arrival procured a ſmall quantity from my druggiſt at London. But, before I begin to give an account of my ſucceſs from this medicine, it will not be improper to inform you of the diſeaſes in which I uſed the quill kind ſince my commencing buſineſs, and the ſucceſs attending it.

In 1778, intermittents were very frequent, the quill bark cured twenty-two of twenty-five, vomits being premiſed, and the primæ viæ cleanſed from hardened fæces. The other three were ſo coſtive, that although I mixed rhubarb along with it, the ague returned. I tried ſtrong decoctions of chamomile tea, to the quantity of eight pints *per* day, which, in a few days, radically cured two of them. The other one was attacked with icteric ſymptoms, and died dropſical.

The confluent ſmall-pox were very frequent at the above period. Two out of ſix died, that got no aſſiſtance. When called, at whatever ſtage of the diſeaſe, after giving a glyſter,

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I gave decoctions of the bark, if the patient were young or delicate; but in ſubſtance, if more robuſt, mixed with aq. cinnam. ſimp. if laxative; and in whatever ſtate their bowels were, always with elix. vitrioli. Of eighty patients I was called to, five died; one of whom was ſeized with a gangrene on the face; another relapſed on the twentieth day, and died of a colliquative diarrhœa, which no medicine could aſſuage. The other three were not particularly affected with the bark, nor had it any power in throwing out the flattened and conglomerated ſmall-pox. They died early in the ſecondary fever.

In periodic pains in the face and temples, which are a ſpecies of chronic rheumatiſm, Peruvian bark joined with valerian, cured two of three patients. The other ſuffered from a carious tooth. In 1779, of twenty-five that had the confluent ſmall-pox, three were blended with purple ſpots, which in one appeared two days before the eruption; in the other two the ſpots came out at the ſame time with the ſmall-pox. From their firſt appearance I plied the patients with bark, elix. vitrioli, and Port wine, while the fever was low, and gave the bark by injection in a decoction of chamomile;

bathing the legs and arms twice a-day with a decoction of the ſame about the warmth of new drawn milk. They all recovered.

One of the other children died of a gangrene of his leg on the twenty-fifth day: and other three died of the ſecondary fever, though every application was adminiſtered that I could think of.

In the ſame year, nine patients were attacked in autumn with the dyſentery. After evacuating the inteſtinal canal with ipecacuanha, or vitrum antimonii ceratum, (premiſing blood-letting if called in time), I gave the bark with chamomile tea, as I found that wine heated the patient. Five recovered by this method. In two others, the villous coat of the inteſtines was diſcharged by ſtool, and every medicine of uſe in that diſorder proved ineffectual. The remaining two, after finding little benefit from what I ordered them, took the root of tormentil, boiled in milk, and, by that means, and a regular diet, braved the diſeaſe. Perhaps the root of tormentil, if given immediately after cleanſing the primæ viæ, would ſuperſede the uſe of opiates. I have had but few opportunities to aſcertain the good qualities of that medicine in the above diſeaſe.

The hoopingcough was endemic in 1780-1, which was relieved by vomits and laxatives, till every ſymptom of pyrexia was gone; then the bark and cold bathing proved a certain and effectual remedy. I gave bark with camphire, as a preventative, and few were attacked that I could prevail upon to perſevere in that mode for any length of time. I have alſo tried the bark in remittents, with good effects. In ſome caſes it griped the patients, and was obliged to be laid aſide; and although I am not ſuch a ſtrenuous advocate for it as ſome of our modern writers, giving it where they can obſerve no viſible remiſſion; yet I have frequently given it in fevers of the continued type as ſoon as I could obſerve an exacerbation, and with advantage.

In two caſes it ſtopped a mortification in cachectic habits; has often been beneficial in the ſlow and nervous fever; but was always hurtful where any topical inflammation exiſted. As alſo in obſtructions of the viſcera. I have tried it in the phthiſis pulmonalis, where the matter expectorated was thin and acrid; it thickened the matter and blunted the acrimony, but generally griped the patient, and increaſed the hectic fever.

The red bark recommended by Dr Saunders, and undoubtedly the bark moſt uſed in Europe ſince its firſt diſcovery, far outdoes the other in efficacy. As I eſteem experiments tried on human bodies preferable to pharmaceutical inveſtigation, (as we are yet uncertain how medicines are acted upon in the body), I ſhall briefly relate the caſes that have come under my cogniſance.

In autumn 1782, I had eight patients with intermittents. Four were cured with the red bark; two with the quilled, though it took one third more to perform the cure. One took four ounces of quilled bark, which changed the diſeaſe from a quartan to a tertian, and was afterwards cured by half an ounce of the red bark. The other was cured with large quantities of chamomile tea. In ſpring 1783, a patient who had taken ſix ounces of the quilled bark, by order of another ſurgeon, applied to me, and was cured by an ounce and a half of the red. Four more were cured by the ſame bark, one of which had a ſevere cough during the whole of the diſeaſe, with difficult expectoration. It acted as a pectoral and ſedative in that patient. In July, ſlow nervous fevers were frequent, accompanied with great proſtration of ſtrength and fainting fits. The exacerbations could not be diſcerned neither from urine nor pulſe. Bark and Port wine were the only exhilarants and tonics uſed with advantage. Thoſe treated with the red bark ſooner recovered their former ſtrength, though no criſis could be perceived.

In two caſes of obſtructed menſes, where the patients ſeemed cacochymical, by giving the red bark, with ſteel, they both recovered, and became regular in two weeks time. I gave it alſo in the phthiſis pulmonalis, with elixir of vitriol: it did not increaſe the hectic, but leſſened the appetite. I gave it to two patients ſubjected to periodical returns of the chronic rheumatiſm, with good effect: the returns have been prevented, and the patients enjoy a better ſtate of health than for ſome time paſt.

I have this autumn given it in the dyſentery, which has been very frequent here; and have not found it fail in one caſe. Premiſing gentle vomits of ipecacuanha or vitrum antimoniacum ceratum, until the feveriſh ſymptoms abate; and, in ſome caſes, promoting a gentle diaphoreſis by opium and antimony; I then give it in ſubſtance, (half a drachm three, four, and ſometimes five times a-day), cauſing the patient to drink large quantities of chamomile tea.

On the whole, the red bark is more certain than the quill kind. It ſeldom or never gripes the patients; and a ſmaller quantity will always perform a cure.

The ſmall circle of my practice has furniſhed me with few obſervations in proportion to thoſe of more extenſive buſineſs; but, at the ſame time, it has enabled me to pay particular attention to thoſe that have come under my care.

VIII.

The Hiſtory of a Caſe in which a large Wound of the Abdomen, with a remarkable Protruſion of the Inteſtines, terminated favourably. By Dr Thomas Cochrane, *Phyſician in St Chriſtopher's.*

IN July 1778, a young negro man in the iſland of St Chriſtopher's, ſtabbed himſelf with a knife in the belly, about three inches above the navel, on the left ſide. An expert ſurgeon, who attended the plantation, being called immediately, found him in the hoſpital, with a large portion of his bowels, which had protruded through the wound, lying on the boarded cabin. Having dilated the orifice, he attempted to reduce the inteſtines to their proper ſituation, but in vain. The obſtinacy of the patient was ſuch, that no ſooner were they replaced, than he immediately counteracted the efforts of the ſurgeon; by which means, being at laſt out of temper, his ſtrength exhauſted, and the patient continuing determined to put a period to his life, the attempt was given up. The manager knowing the worthleſſneſs of the negro, locked him up, and, in a great meaſure, neglected him for twenty-four hours, thinking his recovery impoſſible.

The ſurgeon, on viſiting him next day, finding no alteration in the fellow's reſolution, was not a little ſurpriſed to find, that although the inteſtines had been expoſed for ſo long a time to the external air, yet no alteration of colour had taken place, nor had he the leaſt degree of fever. He prevailed upon him to ſuffer a bandage to be applied to ſupport the bowels, and left him to nature. At this time, prompted by the novelty of the caſe, I went to viſit him, and found the contents of the bandage were the bulk of a child's head.

Some days afterwards, in my way to viſit a patient, I was aſtoniſhed to meet the negro walking to town, which was two miles and a half from his reſidence, ſupporting the protruded inteſtines in a coarſe woollen blanket. And having the curioſity to examine the parts, I was ſurpriſed to find that granulations of fleſh were extending from the wound along the whole ſurface of the guts; and that this was the ſecond or third time he had ſet out on the ſame expedition ſince I ſaw him. But what to me was a matter of greater ſurpriſe, I found that he had gone to bathe in the ſea, and was again to repeat it in that condition. I endeavoured to perſuade him to return, but in vain: he continued his route in a remarkably ſultry day, ſwam in the ſea, and walked back to the plantation immediately after. From this time, the cure went on rapidly; the granulations advanced, a ſac was formed, and, in a few weeks, the whole was cicatriſed. I examined him ſome years after, found him as ſtrong and healthy as ever. He could undergo any labour; and had no other inconvenience than ſupporting the tumour, which reſembled the mamma of a woman.

The very extraordinary efforts of nature in this caſe, afford a convincing proof that too much may be ſometimes done to relieve the patient; and that, by leaving her to herſelf, cures may be completed even without art. It is probable that the rough application of the woollen induced inflammation; but if any practitioner were to preſcribe ſea-bathing under ſuch circumſtances, what would his brethren or the world think of him? Did the ſea-water act as an antiſeptic? or are ſimple wounds of the abdomen, where the inteſtines are unhurt, to be conſidered as being in general ſo dangerous as we are taught to believe? I have known ſeveral inſtances of mules being gored by cattle, where the bowels have fallen to the ground, and the owner having ſecured them, reduced the inteſtines, and ſtitched up the external teguments, without any bad conſequences.

I have had ſeveral caſes where the inteſtines were even wounded, and the patient did well. Theſe cures I chiefly attributed to the loweſt poſſible regimen, and the moſt ſimple dreſſings.

In April 1780, I was called to a ſoldier's child, belonging to the 55th regiment, who had been born 38 hours, but had not been at ſtool. On examining the infant, I found the belly much diſtended, but no appearance of an anus. On the contrary, the parts were perfectly ſmooth, and a ſwelling the ſize of a hen's egg, projected from the fundament. On touching the centre, I could perceive a ſoftneſs where I judged the end of the rectum to be. The child at this time was in the utmoſt agony, had been conſtantly crying, and never ſlept from the moment of its birth.

I plunged a ſmall lancet into the tumour, and, in an inſtant, an immenſe quantity of meconium ruſhed out, along with a great diſcharge of air; and before half the contents were evacuated, the child dropped faſt aſleep. I introduced a tent into the orifice, which I continued for a ſhort time; and the child had the perfect uſe of the rectum afterwards.

IX.

Remarks on the Sigualtian Operation, extracted from a Letter to Dr Duncan. *By Dr* J. H. Myers *of London, written from Paris.*

I Quitted England with every poſſible prejudice againſt this operation, from the influence the opinion of my maſters had on me. But I am at preſent "nullus addictus jurare in "verbis magiſtri." I have ſeen the operation twice performed, with every poſſible ſucceſs. The laſt patient (while I write) is in the room, coming to ſhew herſelf, in juſtice to her operator. It is only eighteen days ſince the operation was performed, and ſhe is in perfect health, by no means injured by the operation. Her former child was extracted by the forceps. The impoſſibility of her being delivered the natural way, was agreed on in the preſence of ſeveral ingenious men in the medical line. The child was delivered by means of this operation; and, we were ſorry to find, dead. The umbilical cord was twice twiſted round the neck of the fœtus; and that part which was round the neck was white, and had the appearance of a ligament, the blood being regurgitated in that part of the cord towards the child. The child's face was extremely red; and, it is probable, from the efforts the fœtus muſt have made, it died apoplectic. This, you will evidently obſerve, depended not on the operation, but happens every day in natural deliveries.

Where there is a phyſical impoſſibility to deliver a woman naturally, the obſtetrical art has only offered us (till this ingenious diſcovery) the Cæſarian operation. In ſpite of the ſucceſs this operation has met with, the greateſt advocates for it cannot but acknowledge the misfortunes that have almoſt always attended the wretched, that have had the courage to ſubmit to it. Theſe dangers alone have been ſufficient to intimidate the moſt ſkilful hand. How little ſurpriſing is it then, ſo few are willing to ſubmit to it, conſidering how few practitioners will venture to undertake it. In theſe circumſtances, the manœuvres uſed, the inſtrument being even in the hand of the moſt ſkilful, tends only to kill the child dans le corps d'une femme vivante, to extract it with violence, or to bring it away piece-meal, after having expoſed the mother (if ſhe yet exiſts) to the moſt horrid torments, as well as the moſt dreadful diſappointment. From ſuch dreadful circumſtances, and from the want of an operation better calculated to meet with ſucceſs, the mother-country has been deprived of many a ſubject, and many a family of their only hopes. How uſeful ſuch an operation, which tends to alleviate theſe miſfortunes! How valuable ſuch a man, who, by application and genius, diſcovers the means to render mankind leſs miſerable, and adds to the riches of his country, by the preſervation of its individuals!

I ſhall attempt to deſcribe to you, as clearly as I am able, the method of performing this operation, as practiſed by the inventor. The woman being placed in ſuch a poſition as is convenient, and other neceſſary precautions being taken, the common teguments of the abdomen above the pubes muſt be ſtretched by means of the thumb and finger of the left hand. The diſſection is then made with the other, by means of an inſtrument much like our ſcalpel, the common teguments and adeps being cut from the ſuperior part of the ſymphyſis, "uſque ad magnorum

"gnorum commiſſuram labiorum pudendo"rum;" an operation by no means very painful, the ſymphyſis then becomes expoſed. A ſcalpel ſomewhat lenticular is to be made uſe of, after the diviſion of the pyramidal muſcles and linea alba. Into this aperture, the index of the left hand is to be introduced, and the ſection of the ligament and cartilage is to be continued. The moment the diviſion is made, there is an enlargement of the pelvis; I venture to ſay, to any extent deſired. The laſt I ſaw was three inches *. The child preſenting the head, the delivery happens in an inſtant, with ſuch quickneſs that it reſembles an exploſion. The child preſenting in any other manner, the feet are to be ſought for in the ordinary way, directing always the great diameter of the head towards the great diameter of the baſon. The wound is then to be filled with tow, and covered with a narrow fourfold bandage. This is fixed by a large piece of linen faſtened about the groin, with ſmall bandages cut from the ſame, or ſewed to it, to keep it tight. The ends of this piece of linen are to be divided, and

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* Accurately meaſured by an inſtrument called *pelvimetre*, contrived by Mr Trainel.

and croſſed over the ſymphyſis, to form what is called by ſurgeons here *le bandage uniſſant*. The patient's linen is to be changed, and ſhe is to be put to bed, well furniſhed with napkins, and lint, *&c.* to abſorb the moiſture. Theſe may be removed and renewed without inconvenience. A hollow is to be made in the bed where the linen is placed, to correſpond with the baſon. A ſlight bandage is to be paſſed croſſwiſe about the knees, at a proper diſtance, to prevent their ſeparation. The head and back are to be ſomewhat raiſed by pillars that do not yield too much. In the following dreſſings the greateſt cleanlineſs is to be obſerved. To prevent any clot of blood remaining in the wound, it is to be ſyringed two or three times. Nothing elſe is to be uſed for the cure of the wound, but a pledget dipped daily into the white of eggs, beat up with brandy. A proper attention to regimen, in the largeſt ſenſe, is neceſſary. Glyſters are to be given occaſionally. The mother may nurſe her child. With theſe and other neceſſary attentions, the patient may be taken out of bed at the end of a fortnight, the union being then generally completed.

The length of the inciſion does not exceed three inches, and the operation does not laſt five minutes. Compare that with the cruelty of the Cæſarian operation, a wound of above nine inches, the viſcera uncovered. In ſhort, the more I reflect, the more I congratulate myſelf on the happineſs of having been eye-witneſs of the one and the other.

On other parts of the continent *, this operation has been tried with equal ſucceſs. Several in Holland have performed this operation with every ſucceſs poſſible. I am hopeful it will be ſoon tried in Britain, where the ſpirit of emulation, and the deſire of ſerving ſociety, is ſo remarkable.

The inſtance I mentioned to you in my laſt, of a wretch not thirty inches high, producing a child of twenty, by means of this operation, leaves no doubt of the poſſibility of delivering a woman in any circumſtance. The diſſection of this woman, who died of a gangrene in the region of the ileum, diſcovered the amazing malconformation of the pelvis, which I meaſured, and is ſtill in the poſſeſſion of Dr Sigault. That the

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* At Mons three or four times. By Camper in Holland once or twice.

the head of a child ſhould have paſſed a baſon not two inches diameter, and otherwiſe horridly deformed, without any laceration being cauſed, or the ſlighteſt mark of contuſion on the cranium, or the leaſt force being uſed, compared with the force uſed by accoucheurs in delivering by means of the feet, or by means of the forceps. This is then the idea to be formed of this delivery; a mother ſo prodigiouſly deformed as always to be obliged to uſe crutches, bent almoſt double, carries a child of two-thirds her ſize. The diſtenſion of the uterus muſt have been amazing, and the mal-conformation of the left ſide of the baſon muſt have been the cauſe of the pains ſhe felt before delivery, and the cauſe of her death. The child, being ſtrongly compreſſed on all ſides, ſcarcely could vegetate in the uterus, which might have cauſed its death.

On the whole, I think this ſubject proves ſufficiently to what extent the advantages of this operation may be carried. I think nature, in ſome meaſure, marks the way to this operation, ſince in difficult deliveries there often happens a relaxation of the ligaments of the pelvis, and even ſometimes a laceration. The objects of this operation, are to augment the capacity of the pelvis in the enlargement of its circle, to procure a ſeparation of the oſſa pubes in dividing the ſymphyſis that unites them, to procure to the fœtus a ſpace ſufficient for its paſſage, to mitigate the cruel pains of the mother, and to preſerve the life of both the one and the other; objects, I think, of the utmoſt importance.

X.

Obſervations on the Uſe of the Cuprum Ammoniacum, in the Cure of the Chorea Sancti Viti. Communicated to Dr Duncan, *by* Joſhua Walker, *M. D. Phyſician to the Leeds Infirmary.*

THE requeſt which my worthy and much reſpected friend Dr Duncan has made, in the preface to the ninth volume of the Medical Commentaries, namely, That the Faculty would aſſiſt him in collecting materials for the tenth, has induced me to preſent him with the following remarks upon a diſeaſe which has often been found tedious and uncertain in its cure. If he ſhould think them in any degree worthy the attention of the public, he is at liberty to communicate them; but, if otherwiſe, to ſuppreſs them, as he may judge proper, as I am no ſtranger to his candour and diſcernment.

The ſuperior advantages of the cuprum ammoniacum, as a tonic, in the cure of epilepſy and convulſion, firſt led me to try its effects in a caſe of chorea, which had reſiſted a courſe of the uſual antiſpaſmodics, and even of the bark in full doſes. This patient was about ſeven years of age, of a thin extenuated form, but who, previous to the attack of this diſeaſe, had enjoyed a good ſtate of health, and was remarkably active and ſprightly in his diſpoſition. After he had taken the cuprum about a fortnight, the ſymptoms began to decline, and in ſix weeks he was perfectly cured.

The ſucceſs which had attended the uſe of this remedy in the foregoing inſtance, induced me to make farther trials of it in caſes of a ſimilar nature; and I have preſcribed it, during the laſt eight or nine years, with the ſame happy conſequences, to a variety of patients labouring under the chorea; and, except in one inſtance, which gave way to large doſes of vitriolic æther, I have never known it fail.

But although my expectations have ſo ſeldom been diſappointed, when the cuprum has been well prepared, and properly adminiſtered, yet ſome caution is required in its uſe; and in many caſes it ought not to be preſcribed, till the patient has been prepared for it by ſuitable evacuations, low diet, *&c.* Without entering into the theory of convulſions, it will here be ſufficient to remark, that the chorea ſeldom attacks children under ſeven or eight years of age, and as rarely after the period of puberty. In this interval, the ſyſtem is particularly irritable, and liable to convulſive affections; and while in ſome, who are attacked with the chorea, a plethoric habit evidently prevails, in others, a peculiar delicacy of conſtitution is obſervable, accompanied with ſymptoms of general relaxation and debility. It is chiefly to patients of the latter claſs, that the foregoing remedy has been ſucceſsfully adminiſtered. In order, therefore, that the ſame advantages may be obtained from it, in thoſe of full plethoric habits, and robuſt temperaments, it is neceſſary that they ſhould be brought to nearly the ſame level, prior to its uſe. This may be ſafely, and, in general, eaſily accompliſhed, by directing the patient to abſtain, in a great meaſure, from animal food and fermented liquors; by interpoſing occaſional bleedings and mild cathartics, at proper intervals, and ſo regulated, that the patient may eaſily bear the evacuations without inconvenience; for, perhaps, we ought always to be cautious, how we reduce the ſtrength ſuddenly in irritable habits. I think I have ſeen diſadvantages ariſing from it, which might poſſibly have been avoided, and the intention obtained with greater certainty, and leſs hazard, by allowing more time, and by ſtrictly obſerving the uſe of a low ſparing diet, conſiſting chiefly of milk, farinaceous vegetables, and a very moderate quantity of animal food at dinner only.

When the plethoric ſtate is taken off by theſe means, joined to bodily exerciſe, or riding on horſeback, which ſhould be ſo long continued as to bring on ſome degree of fatigue, we may begin the uſe of the cuprum, in doſes of half a grain, repeated three times a-day; which, as the ſtomach becomes accuſtomed to it, ſhould be gradually increaſed till a nauſea be excited, which will generally be the caſe when three or four grains are taken at a time. If there ſhould be no remiſſion of the ſymptoms during the firſt two or three weeks after the remedy has been given in full doſes, the patient ſhould ſtill perſevere, obſerving, at the ſame time, to guard againſt the plethoric ſtate, if it ſhould be found neceſſary, by repeating the cathartics, and by paying a ſtrict attention to temperance and regularity. Some caſes require the medicine to be continued for ſix or eight weeks, before the cure be confirmed, and in ſome few a little longer.

Patients of weak debilitated conſtitutions, thin habits, and delicate nerves, require little preparation. A doſe or two of very gentle phyſic, if coſtiveneſs ſhould make it neceſſary, may be adminiſtered; but briſk purging, in ſuch caſes, generally does harm, and renders the nervous ſyſtem ſtill more irritable.

The cuprum ammoniacum which I have uſually preſcribed, has been prepared according to the formula given in the Edinburgh Pharmacopœia publiſhed in 1774. In the laſt edition ſome alteration has been made in the preparation, perhaps for the better, and the proceſs is far more eaſy and expeditious.

The moſt convenient form of adminiſtering this remedy, is in pills; and the Pharmacopœia before mentioned, ſupplies us with an elegant and efficacious formula, under the title of *pilulæ cæruleæ, vel e cupro*, viz.

℞. Cupri ammoniaci grana ſexdecim,
Micæ panis, ſcrupulos quatuor,
Spiritus ſalis ammoniaci, q. ſ.
Fiat maſſa, dividenda in pilulas triginta duas æquales.

XI.

The Hiſtory of a Caſe of Ileus, terminating fatally, with an Account of the Appearances on Diſſection. By Dr James Gerard, *Phyſician in Liverpool.*

H—— B——, a married woman, of a plethoric habit, about thirty years of age, ſent for me on the 21ſt July. She complained of coſtiveneſs, with ſlight pains in the bowels, which had ſubſiſted ſince the 15th. She perceived no ſhiverings, the pulſe and tongue were in a natural ſtate, and the ſkin ſoft. I ordered her an infuſion of ſenna with Glauber's ſalt. On the 22d it had produced no effect; caſtor oil, and purgative glyſters were there-

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fore ordered, and about ten ounces of blood were taken from the arm. Notwithſtanding theſe means, ſhe found no relief. The blood ſhowed no marks of inflammation; and the obſtruction of the bowels ſtill continued. On the 23d, the glyſters were continued; and, by her own deſire, ſhe drank largely of ſenna tea. On the 24th, ſhe complained of ſickneſs; the ſenna tea had been rejected, and no paſſage was procured. Ten grains of calomel, and a ſcruple of the powder of jalap, made into pills with a little mucilage, were ordered to be taken at twice; frequent doſes of ſalt of tartar with lemon juice, were given, and the tepid bath was uſed. Not finding the ſmalleſt relief, the patient was again bled on the 25th; the calomel and jalap were repeated, a bliſter was applied upon the abdomen, and the tobacco-ſmoke was injected into the rectum; but all to no purpoſe. The pain in the abdomen increaſed, particularly upon preſſure; the vomiting became more conſtant and oppreſſive, attended with reſtleſsneſs, anxiety and thirſt. The pulſe continued pretty firm, and not more than ninety in the minute, and the tongue ſtill clean.

Having informed myſelf that ſhe was advanced betwixt the fourth and fifth month of pregnancy, I was ſuſpicious that the inſuperable difficulty to obtaining ſtools, might be occaſioned by the retroverſion of the uterus; but underſtanding, at the ſame time, that there was not the ſmalleſt obſtruction to the flow of urine, (one of the pathognomonic ſymptoms of that diſeaſe), I could not think it to be a true and complete retroverſion. When I reflected, however, that the means we had uſed, had not produced the ſmalleſt effect, and had learned that there had always been ſome obſtacle to throwing up the glyſters in any conſiderable quantity; I ſuſpected that there muſt be ſome mechanical obſtruction to the paſſage through the inteſtine, which poſſibly might be occaſioned by ſome particular ſituation of the uterus, approaching to a retroverſion. I therefore thought it prudent to ſatisfy myſelf of the true ſtate of it by an examination; which being complied with, I did not find that I could, in the ſmalleſt degree, attribute the difficulty to any preternatural ſituation of that organ. I was, however, ſenſible of a ſubſtance being in the rectum, which was very perceptible to the finger

finger through the vagina, and I thought it to be hardened fæces. With a view to remove this, I ordered, on the 26th, glyſters of warm water and oil, to the quantity of two or three pounds, to be thrown up, if poſſible, by a ſyringe; and I repeated the pills with calomel and jalap. Not one pound, however, of the warm water could be thrown up, for it returned by the ſide of the pipe, although that was wrapped round with lint, to plug up the rectum. On the 27th, a glyſter of olive oil only was thrown up, with a view of facilitating the deſcent of the hardened fæces, as I ſuppoſed: but this having no better effect than the reſt, I introduced my finger into the rectum, to judge whether it could be ſcooped out; and I found that what I ſuppoſed to be fæces, was not ſo, but was a ſubſtance ſomewhat firm, appearing to the finger, with which I could but juſt reach it, either fleſhy, or covered every where with membrane. Indeed, I thought the feel was like that which an intuſſuſceptio would give, being ſo that I could paſs my finger all round the end of it, in the centre of which I perceived ſomewhat like an orifice or aperture; yet I thought it lower in the rectum, than I could well conceive that complaint to happen. Suppoſing, however, that this might be the caſe, I attempted to return it by paſſing up the whale bone probe, which I did with ſo much force, as to give ſome pain, but without effect. Her ſituation was now truly deplorable, as it did not appear to admit of relief; it was neceſſary, however, to preſcribe ſomewhat from time to time, by way of placebo, till the 12th of Auguſt, when the aggravation of each ſymptom, pain, anxiety, want of ſleep, inceſſant vomitings, together with exceſſive tumefaction of the belly, (conſtituting what is called a tympany), ſo fatigued and exhauſted her, as to cauſe death.

In the evening of that day, I got permiſſion to open the body. Upon dividing the teguments of the abdomen, the inteſtines preſſed very forcibly againſt the peritonæum, and finding that they could not be included again within the parietes without diminiſhing their bulk, I punctured them in two or three places, and let out, not only a great deal of air, but alſo a large quantity of liquid fæces, and three cherry ſtones; but theſe were at a diſtance from the obſtruction, and did not in the leaſt contribute to produce it. The capacity of the inteſtines was increaſed to ſuch a degree, that the jejunum and ileum were equal to the natural ſize of the colon, and that was enlarged in proportion. The ſtomach was ſmaller than common. After this general view, I took out the uterus, which was evidently impregnated, and in its natural ſituation; but it was remarkably diſeaſed, having a ſchirrous tumour (the ſize of a pigeon's egg) on one ſide of the cervix uteri, and ſeveral ſmall knots on different parts of the fundus. Both ovariæ, and the Fallopian tubes, were alſo ſo diſeaſed as to produce great confuſion of parts; and one of the former contained ſome matter. The fœtus ſeemed to be in a perfectly healthy ſtate, but was ſtrangely entangled with the umbilical cord. I next examined the diſeaſed part of the inteſtine, which was ſituated in the rectum, within the hollow of the ſacrum, juſt in reach of the finger per anum. It did not occupy above an inch of the inteſtine in length, but the whole circular fibres were contracted and thickened, giving the appearance of fungus internally; through the centre of which, after death, I paſſed a piece of wood, about the thickneſs of a ſmall crow quill, with very little reſiſtance, but that of mucus. On one ſide of the gut, externally, the contraction was greater than on the other, producing a knotty ſchirrous firmneſs.

Both this, and the diſeaſed impregnated uterus, are now in the poſſeſſion of Dr Monro, Profeſſor of anatomy in the College of Edinburgh.

XII.

The Hiſtory of a Caſe of Hydrocephalus, terminating ſucceſsfully. By Dr John Evans, *Phyſician in Liverpool.*

MARCH 20. 1784, I was ſent for to Maſter Charles Shipley, a fine boy about the age of ſeven months, ſon to the Dean of St Aſaph. He had hitherto enjoyed an uninterrupted ſtate of health; but, a ſhort time before I ſaw him, was attacked with violent convulſions. For ſome days previous to my viſiting him, his head had been obſerved to have been gradually increaſing in bulk. This circumſtance being communicated to me, I examined it carefully, and was perfectly ſatisfied that the ſagittal ſuture was conſiderably opened; for I could very diſtinctly feel the edges of the parietal bones, and alſo a pulſation all along the courſe of the ſuture. He had likewiſe a remarkably pale countenance, and the pupils of his eyes were much dilated, attended with ſtrabiſmus. Accompanying theſe circumſtances, there was an œdematous ſwelling of the whole ſcalp and face, as the ſlighteſt preſſure of the finger, on any part of the head, cauſed a mark to remain for ſome time. My patient being in this deplorable ſituation, I found myſelf almoſt at a loſs what plan to purſue for his moſt ſpeedy relief. However, it was neceſſary that ſomething ſhould be done immediately; therefore, without loſs of time, the warm bath was made uſe of, a bliſter applied to the nape of the neck, and a purgative glyſter adminiſtered. Theſe applications, in ſome degree, procured a mitigation of his complaints for that night.

The next day, all the alarming ſymptoms that had occurred, returned with equal violence, which induced me to recommend leeches to be applied to the temples, the warm bath to be uſed at the approach of every convulſive attack, and a mixture compoſed of ſpiritus mindereri and tinctura thebaica to be taken every two or three hours. By this mode of treatment, the ſpaſmodic affections became leſs frequent, and of a ſhorter duration; and a moſt profuſe perſpiration of the head took place, which continued for the ſpace of twenty-four hours. From the time the ſweating commenced, there was a gradual diſappearance of all his complaints, till his head was reduced to its natural ſize, which happened in the courſe of three days. And from that period he has been perfectly free from any return of this diſorder. A diſcharge was kept from the bliſtered part for ſeveral months.

XIII.

The Hiſtory of an uncommon Swelling of the lower Extremities in a pregnant Woman, terminating favourably immediately after an Abortion. By Dr John Evans, *Phyſician at Liverpool.*

A Lady of a delicate conſtitution, being about three months advanced in her pregnancy, was (on the 19th of June 1783) ſuddenly alarmed by an uterine hæmorrhage. The next day the diſcharge continued, in ſmall quantity, attended with a ſlight pain in the loins. It was thought adviſable ſhe ſhould let blood. Accordingly about ten ounces were taken from the arm. She alſo took a draught compoſed of the decoction of the red Peruvian bark, with vitriolic acid, every four hours; and, at bedtime, an anodyne. This mode of treatment was purſued for ten days without alteration, excepting the addition of a bolus of nitre and camphire to each draught.

On the 30th ſhe began to complain of her legs and feet being remarkably cold, and of a chilneſs over the whole body. The following morning, ſhe had a pain in the left groin, a ſwelling of the labium pudendi of the ſame ſide, which extended around to the back, accompanied with a dyſuria, a pain and ſwelling down the inſide of the thigh, and the back part of the leg, with ſuch a degree of ſtiffneſs as to prevent the extenſion of the limb, without great uneaſineſs. The leg was kept in the moſt favourable poſition for ſeveral days, and well rubbed with camphorated ſpirits.

On the 9th of July, the ſwelling of the whole limb was increaſed, and had an œdematous appearance, but was harder to the touch than in a common anaſarca. A bliſter was then applied to the upper and back part of the leg, which, in ſome degree, removed the ſtiffneſs and pain. In this ſtate every thing remained for the ſpace of a week, when ſea-bathing was tried, and continued for a fortnight. In three days after the commencement of the bathing, a conſiderable increaſe of the ſwelling of the whole limb was obſerved, which came on every morning as ſoon as the patient was out of bed, but it was attended with leſs pain and hardneſs than before.

On the 27th, (in the afternoon), the ſwellings ſuddenly diſappeared; and the next morning ſevere pains in the back and abdomen took place, with a bearing down reſembling labour. Theſe complaints continued for the ſpace of two hours, at the end of which period ſhe was relieved by a large uterine diſcharge, which, upon examination, proved to be a miſcarriage. Every ſymptom of diſeaſe now diſappeared, and nothing remained but a ſmall degree of ſwelling towards night, which gradually diminiſhed as ſhe gained ſtrength.

N. B. The ingenious Mr White of Mancheſter publiſhed lately an account of the ſwelling, of one or both of the lower extremities, which ſometimes happens to lying-in women; which he imagines is produced by the burſting of a common trunk of the lymphatic veſſels, in conſequence of the preſſure of the child's head in delivery. It is evident from the caſe here related, that the diſeaſe is not peculiar to women in the ſtate deſcribed by Mr White; therefore, the cauſe of this complaint is ſtill obſcure, and is likely to remain ſo, till diſſection affords an opportunity of inveſtigation.

XIV.

XIV.

*Obſervations on the Gaſtric Juice. By * * * Phyſician in London.*

SOME years ago, acidity in the ſtomach was conſidered to be a morbid appearance. But, of late, an opinion has prevailed, that this ſubſtance naturally exiſted in healthy ſtomachs; that diſeaſes were produced by an exceſs or defect of it; and that the effects of many medicines were explicable from the union of them with the acid contained in the human ſtomach. The foundation of this opinion has been probably laid in the experiments of Sir John Pringle and Dr Macbride; which ſhew, that mixtures compoſed of ſuch ſubſtances as are commonly taken into the ſtomach by way of aliment, when placed in a heat of about 98° of Fahrenheit's thermometer, undergo the vinous and acetous fermentations.

Thoſe who have embraced this doctrine, believe that the medicinal effects of the *calces* of metals, when applied to the ſtomach, depend upon their union with the acid contained in it; and that if this ſubſtance does not exiſt there, they are inactive. Upon this ground it was affirmed, that the *calces* of antimony (of one of which James's powder is a preparation) are leſs certain in their effects than the ſaline preparations, as tartar emetic; becauſe, in the former caſe, the efficacy of the antimony muſt depend upon the quantity of acid with which it unites in the ſtomach, the quantity and exiſtence of which are uncertain; but, in the latter caſe, this medicine was more conſtant in its effects, becauſe the antimony is combined with an acid before its application, and may act independently of the acid of the ſtomach.

Magneſia alba is another ſubſtance that has been ſuppoſed to become purgative, only in conſequence of its combination with the acid of the ſtomach. The above opinion appears to be an error, becauſe the concluſion on which it is founded is unwarrantable. For although alimentary mixtures, contained in glaſs veſſels, expoſed to 96° of heat, produce wine and acetous acid, the changes which theſe mixtures undergo, when applied to the action of living powers, may be totally different. I need not give the proofs, that, in the former caſe, all the alterations that can be made in the mixtures, are in their mechanical and chemical qualities; but that, in the latter caſe, they are not only ſubject to theſe changes, but to thoſe of the living powers, which frequently produce ſubſtances that are the peculiar effect of life.

In the next place, it has not been ſhewn by any obſervations and experiments, that the human ſtomach contains acid in a healthy ſtate. Dr Ruſh of Philadelphia, and Dr Penny of Exeter, have indeed ſaid, that, in a few trials, they have found that the food which conſiſted of animal and vegetable matter, vomited a few hours after it had been ſwallowed, contained ſome acid. But this experiment is inconcluſive; becauſe it is not clear that the ſtomach was not in a diſeaſed ſtate, and that the aliment did not contain the vegetable acid, which was only ſeparated from the food during digeſtion: and, at any rate, it only ſhews that an acid may be formed *during digeſtion*, not that it is contained naturally in the ſtomach after the aliment is digeſted.

The coagulation of milk in the human ſtomach, was alſo believed to depend upon the acid it there met with. But Dr G. Fordyce found, that a ſmall quantity of a ſtomach which had been waſhed with a ſolution of alkali, coagulated a very large quantity of milk. When I attended St Thomas's Hoſpital ſome years ago, I procured the ſtomach of a man who had died after being a few hours in an apoplectic fit, previous to which he had been in a ſtate of health. This ſtomach only contained about one ounce of a reddiſh thick fluid, that was not fœtid, but had a ſmell which I cannot compare to that of any other ſubſtance.

To the teſt of the infuſion of archill, and the juice of black cherries, this ſtomach did not contain any acid.

A few drops of the fluid contained in this ſtomach being mixed with about a quarter of a pint of cows milk, did not produce any change for an hour; but in two hours time, during which it was expoſed to about 66° of heat, it was changed into a firm maſs of coagulated matter.

About half a drachm of this ſtomach that had been ſteeped for half an hour in a diluted ſolution of fixed alkali, was thrown into a quarter of a pint of milk, and ſtirred therein. After ſtanding a night in the heat of about 60°, I found part of the milk, immediately ſurrounding the piece of the ſtomach, coagulated. The whole was then ſtirred together, and, in two hours, all the milk was firmly coagulated.

The above experiments were made ſeventeen hours after the death of the patient; and fifty-four hours after this event, I alſo put a ſmall piece (half a drachm) of this ſtomach, which had been twenty-four hours in a large quantity of warm water, into a veſſel containing three ounces of milk. In a few hours a part of the milk was coagulated.

I have repeated theſe experiments, with the ſame event, on ſubjects that died of diſeaſes: from which we may fairly and concluſively deduce, that the coagulation of milk in the human ſtomach is not produced by any acid, but by a fluid ſecreted there, or by the ſubſtance of the ſtomach itſelf.

The effects of the above medicines certainly do not, either neceſſarily or uſually, depend upon their being united with the acid of the ſtomach; although, upon ſome occaſions, it is poſſible they may produce their medicinal effects in a greater degree, and even ſome peculiar effects, in conſequence of being combined with this acid.

Metallic ſubſtances certainly produce their medicinal effects under circumſtances in which there is very little probability of their being combined with this acid, as in the caſe of the external application of the calx of quickſilver; and of this metal, merely divided into ſmall maſſes by means of mucilage, or eſſential oil. And antimony alſo, in its crude ſtate, or when combined with ſulphur, produces all the changes in the body that are effected by the ſaline preparations, or by the calx of this ſemi-metal. Farther, it is a point diſputed by men of equal authority, whether the ſaline preparations, or the calces, corroſions, and metalline ſtates of the metals, and ſemi-metals, be moſt efficacious? It is well known, that theſe ſubſtances, exhibited along with alkalis, or to patients who have, for ſome time before, taken alkaline ſubſtances, or abſorbent earths, prove equally active in the cure of diſeaſes with theſe ſubſtances combined with acids.

The purgative effects of magneſia certainly do not neceſſarily depend upon its junction with the acid of the ſtomach, becauſe it operates in perſons that have no acid in their ſtomach, or that certainly have it in ſo very ſmall a quantity, that the earthy ſalt formed by its union therewith cannot reaſonably be ſuppoſed, on account of its quantity, to prove purgative.

I have known patients who took from thirty to forty grains of volatile alkali, daily, for three, four, and even ſix months; during which time, their food was principally of animal matter, and their digeſtion was as good as in health. Their diſeaſe was the palſy. I have given to theſe patients *magneſia alba* repeatedly, and always found that it proved as purgative as I had reaſon to ſuppoſe it would have done, had this alkali not been taken at all.

However, when any acid is contained in the ſtomach in ſufficient quantity, as perhaps in ſeveral diſeaſes of infants, the *magneſia alba* may prove purgative, in conſequence of its union with this acid, as well as when a quantity of acid is drunk immediately after taking it alone. But theſe facts ſurely do not authoriſe us to conclude, that this earth has not purgative qualities, independently of its forming an earthy ſalt with acid in the ſtomach.

XV.

Account of Appearances on the Diſſection of a Child dying of Hydrocephalus. By Dr Joſhua Dixon *Phyſician, Whitehaven.*

IN the month of May 1784, I was requeſted to pay particular attention to the following very ſingular caſe. A child, aged three months, had, from its birth, been liable frequently to ſuffer a great variety of nervous, principally convulſive affections. They were, for the moſt part, internal and partial, twitching its limbs in a ſlight, ſcarce perceptible manner. The contractions were, however, ſometimes more univerſal and potent, and a conſiderable degree of ſtupor afterwards took place. His bowels were much irritated and exceedingly flatulent. Stools irregular, occaſionally very numerous, and lax, but generally few and conſtipated. To the uſual irritations of pleaſure or pain, this child was perfectly ſenſible. But the ſymptoms moſt obvious and important, were thoſe of hydrocephalus, a preternatural enlargement of the head having taken place, and ſurpriſingly diſtorted its ſurface. The ſutures and bregma appeared remarkably extended, whilſt the bones themſelves were peculiarly ſoft, thin, and tranſparent. In a caſe ſo truly deplorable, there could be no expectation of deriving any permanent degree of relief from the aids of art. Moderate and habitual friction and bliſters, with alternative doſes of the mildeſt mercurials, and leaſt ſtimulant laxatives and carminatives, might perhaps, in ſome meaſure, contribute to prevent the rapid progreſs of the diſeaſe, and in alleviating the moſt urgent ſymptoms, gently ſmooth this avenue of death. Their employment was, however, of no conſequence, and the complaint continued to be manifeſtly aggravated till the 7th of May 1785, when, after having laboured under circumſtances ſhocking to humanity, fifteen months and four days, without any extraordinary recurrence of convulſive or other affection, the child happily expired.

At the judicious requeſt of the parent, (Mr Joſeph Brag, a ſurgeon in Whitehaven), an accurate attention was paid to the ſtate of the parts after death, and the following intereſting pathological facts fully aſcertained. The hair upon the ſcalp was light-coloured, very thin and flaccid; and the bones of the head, eſpecially the temporal and parietal, appeared connected by a large expanſe of pure membranous ſubſtance. The whole circumference of the head, before the water was evacuated, taken in a line with the coronal ſuture, or rather above it, and in that direction, extending on each ſide to the occiput, meaſured two feet and two inches: from ear to ear, acroſs the parietal bones, eighteen inches.

An inciſion being now made by Mr Hamilton, immediately above the occipital bone, ſeven pints and ſix ounces of perfectly clear limpid water were diſcharged, and the contents of the cranium amply diſplayed. With regard to the cerebrum and cerebellum, not the leaſt veſtige of either could be found. They had, upon a more particular examination, become a membranous cyſt for the water; not exceeding the thickneſs of a piece of moiſt parchment; the internal ſurface of which, formed by the medullary ſubſtance, was remarkably ſoft, uniform and ſmooth: whereas the external retained the convoluted appearance of the cortical part of the brain. It is therefore highly probable, that the water had been firſt collected in the ventricles, and from thence extended its influence, and gradually accumulated. The origin and progreſs of the medulla oblongata could with difficulty be accurately traced. Aſſuming a very firm fibrous appearance, it was manifeſtly contracted and diminiſhed. The dura and pia mater were a little thickened, but in other reſpects perfectly natural.

We ſhould not, from an attentive regard to the peculiar circumſtances of this diſeaſe, be led to coincide with the general ſentiment of phyſiological writers, ſince we can by no means poſitively conclude, that the brain, as the origin of the nervous ſyſtem, can ſo powerfully influence the whole animal œconomy, when we find an inſtance where ſenſe and motion were neither altogether interrupted, nor remarkably impaired during the courſe of a tedious indiſpoſition, and yet the brain itſelf was ſo far totally obliterated, that a very ſmall, ſcarce perceivable portion of medullary, or cortical ſubſtance, could be diſcovered.

I have ſince had an opportunity of attending a ſimilar caſe of hydrocephalus, the ſymptoms of which did not take place before the third, and became fatal in the ninth month. They appeared certainly to originate from a concourſe of moſt violent nervous affections, which firſt preceded, and afterwards accompanied them. Frequent and powerful convulſions, with conſequent ſtupor and inability for motion were daily experienced; but neither in this, nor the former caſe, did the eyes appear diſtortedly prominent. The external and internal remedies uſually employed, eſpecially thoſe chiefly recommended by modern practitioners, *viz.* bliſters and mercurials were altogether inefficacious, though the former had been long kept open, and the latter cautiouſly puſhed for a conſiderable length of time, ſo as gently to excite a ſalivating influence. From the occaſional exhibition of aperients and carminatives, a temporary relief to the moſt painful ſymptoms was, however, derived. Relative to the appearace of the cranium, it was by no means ſo remarkably diaphanous, nor had it ſuffered a degree of expanſion equal to the preceding caſe. The quantity of water contained in the head could not be aſcertained, as the parents would not permit it to be afterwards drawn off.

SECT. III.

Medical News.

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IT is now many years ſince attempts have been made to eſtabliſh a regular ſchool of phyſic in Dublin. But theſe cannot be ſaid to have been hitherto attended with the deſired ſucceſs. Indeed ſeveral of the moſt important profeſſorſhips, though endowed with ample ſalaries, might be conſidered as merely nominal. But the Legiſlature of Ireland, animated by a due zeal for the intereſt of the nation, and for the intereſt of ſcience, have, in their laſt ſeſſion of Parliament, paſſed an act which bids fair to put medical education, in the

Univerſity of Dublin, on a much more uſeful footing than formerly. The nature and tendency of this act will be readily underſtood from the following advertiſement, which has been lately inſerted in the Dublin newſpapers.

"Whereas, by an act of Parliament paſſed in Ireland this ſeſſion, entitled an act for eſtabliſhing a complete ſchool of phyſic in this kingdom, it is enacted, that ſeveral profeſſorſhips ſhall be eſtabliſhed, namely, a profeſſorſhip of the inſtitutes of medicine, a profeſſorſhip of the practice of medicine, and a profeſſorſhip of materia medica, pharmacy, and natural hiſtory; and that the three profeſſors of the ſaid ſeveral branches, ſhall be called the King's Profeſſors in the city of Dublin, on the foundation of Sir Patrick Dunn; and alſo that three lecturerſhips, long ſince eſtabliſhed in the Univerſity of this kingdom, for the learning of anatomy and ſurgery, chemiſtry and botany, ſhall be univerſity profeſſorſhips in the ſaid ſeveral branches reſpectively; and that the ſaid ſeveral profeſſorſhips ſhall have perpetual continuance.

"Notice is hereby given, that the election of the King's Profeſſors in the city of Dublin, on the foundation of Sir Patrick Dunn, will be held at the houſe of the Provoſt of Trinity College, Dublin, on Tueſday the 6th of December 1785, after ſuch examination as the electors may require, which will begin at the hour of ten o'clock in the morning of the ſaid day.

" The following emoluments and advantages attend theſe profeſſorſhips.

" Each profeſſor will receive the yearly ſum of L. 100, to commence at the time of his election; and he may alſo charge reaſonable fees, to be paid by all ſuch perſons as attend the reſpective lectures; the ſaid fees to be paid on admiſſion, and to be from time to time regulated by the King and Queen's College of Phyſicians in Ireland.

The lectures of each profeſſor are to commence on the firſt Monday in November, and to continue until the end of April; and are to be given four days in the week, at leaſt, in Trinity College, Dublin. They are to be delivered in the Engliſh language, unleſs it be otherwiſe ordered by the Preſident and Fellows of the ſaid College of Phyſicians. Clinical lectures will be eſtabliſhed, and every aſſiſtance and encouragement given for the ſucceſs and advancement of medical inſtruction, in the ſaid ſchool of phyſic.

"The profeſſorſhips are open to Proteſtants of all nations, provided they have taken medical degrees. Each profeſſorſhip ſhall become vacant at the end of ſeven years from the date of election; but at the expiration of the ſaid ſeventh year, the ſame profeſſor may be again elected. The electors are bound to the ſtrictest impartiality, by an oath, required by the ſaid act to be taken by every elector, previous to the election.

"It is deſired, that all perſons intending to offer themſelves as candidates for any of the ſaid profeſſorſhips, do, within two months from the date hereof, ſend their names, and alſo an account of the places of their education, of the univerſities in which they have taken their medical degrees, and of the places in which they have practiſed, to the Reverend Henry Dabzac, D. D. Trinity College, or to Stephen Dickſon, Eſq; M. D. Peter-ſtreet, Dublin, who will give any farther information on the ſubject that may be neceſſary.

Signed

Signed by order,
Henry Dabzac, Regiſter of Trinity College.
Stephen Dickson, Regiſter of the College of Phyſicians."

Dublin, 3*d September* 1785.

Although, by the above advertiſement, the election was to have taken place on the 6th of December, yet, in conſequence of ſome informality with reſpect to the day, it is now delayed, we hear, till the 21ſt of March 1786.

* * * *

On the 3d of April 1785, died at Oxford, Dr John Parſons, a phyſician of diſtinguiſhed eminence, and Profeſſor of Anatomy in the Univerſity there. He was the ſon of Major Parſons of the dragoons, who reſided principally in Yorkſhire; and in that county Dr Parſons was born in the year 1742. He had his early education at Weſtminſter ſchool; from whence, in the year 1759, he was elected to a ſtudentſhip in Chriſt-church College, Oxford. He made choice of medicine as his profeſſion, and proſecuted the ſtudy of it with uncommon aſſiduity, not only at Oxford, but alſo both at London and Edinburgh. But while he beſtowed much attention on every branch of medical knowledge, he at firſt ſhewed a particular predilection for natural hiſtory and botany. In the latter branch, in particular, he made a very diſtinguiſhed figure during his ſtay at Edinburgh. And in the year 1766, he had the honour of obtaining the prize medal, given by Dr Hope, for the moſt extenſive and elegant hortus ſiccus.

This, however, was only a prelude to more diſtinguiſhed honours, which were ſoon afterwards conferred upon him. The munificence of the late Dr Lee of London, had founded an anatomical profeſſorſhip in Chriſt-church; and in 1767, Dr Parſons was elected the firſt profeſſor, although he had not then been of ſufficient ſtanding to obtain any degree in medicine. In conſequence of this appointment, his attention, it may naturally be ſuppoſed, was more particularly directed to anatomy. Under his direction, a very commodious anatomical theatre was built at Oxford. And for the inſtruction of his pupils, he provided a ſet of anatomical preparations, which, for neatneſs and elegance, have ſeldom been ſurpaſſed. From the time of his appointment to this profeſſorſhip at Oxford, he read two courſes of anatomical lectures every year. And although they were calculated rather for the general philoſopher, than the medical practitioner, yet they were not only highly inſtructive to all his audience, but afforded to every diſcerning ſtudent inconteſtable evidence both of the genius and abilities of the profeſſor.

In the year 1769, he took the degree of Bachelor of Medicine, and was very ſoon afterwards elected one of the phyſicians to the Radcliffe Infirmary at Oxford. In the year 1772, he obtained the degree of Doctor of Medicine. And theſe academical honours were ſoon followed by more than an ordinary ſhare of private practice: For from his attention and ſucceſs as a phyſician, his reputation with the public in general, kept pace with the eſteem in which he was held by the Univerſity. But although he was ſoon introduced into very extenſive practice, yet the activity of his diſpoſition led him to conſider himſelf as not unequal to ſtill farther toils. Accordingly, in the year 1780, when the eſtabliſhment of a clinical profeſſorſhip in the Radcliffe Infirmary, endowed with an ample ſtipend by the late Chancellor of the Univerſity, Lord Litchfield, was finally ſettled, Dr Parſons was unanimouſly elected the firſt profeſſor. In this department alſo, he read lectures during the winter months, with much credit to himſelf. But it is not improbable, that the various active employments in which he was engaged, and which neceſſarily expoſed him to fatigue and danger, had ſome ſhare in overthrowing a conſtitution naturally ſtrong. He was not, however, cut off by any tedious or painful ailment, but died of a fever in the 44th year of his age.

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On the 24th of July 1785, died at London, Dr Richard Huck Saunders, a phyſician of deſerved and diſtinguiſhed eminence. He was the ſon of —— Huck, Eſq; who poſſeſſed, in the county of Weſtmoreland, a ſmall eſtate, which had been in his family for many generations. Dr Huck was born there in the year 1720; and not long after he was born, he loſt his father. Although his mother, whoſe name was Harriſon, ſurvived her huſband for ſome years; yet his education was chiefly conducted under the care of a maternal uncle. By him he was ſent to the grammar ſchool of Crougland in Cumberland, which was at that time much celebrated in the north of England.

There

There he received the rudiments of a claſſical education, and made very conſiderable proficiency in the Latin language; ſo that, during the whole future courſe of his life, he was able to compoſe in that language with much facility and elegance.

He at firſt intended to purſue his claſſical education under the direction of another uncle, who was a Fellow of one of the Colleges at Oxford. But this ſcheme was interrupted by an accident which ſoon afterwards happened to him. In conſequence of a fall from a hay-cart, one of his legs was very much hurt; and he received, as he always thought, a longitudinal fracture of the tibia. This accident occaſioned to him a very tedious confinement. It was followed by a carious ulcer, and a number of ſucceſſive exfoliations. Theſe continued even to haraſs him at different periods, during the whole remainder of his life.

By this accident, however, he was led to think of a change of his intended profeſſion, and to apply himſelf to ſurgery. In the proſecution of this plan, he was put under the tuition of Mr Neal, a ſurgeon and apothecary at Penrith, with whom he remained for five years. He then went to London, that he might proſecute his ſtudies with greater advantage. And he became a dreſſing pupil at St Thomas's Hoſpital, under Mr John Girle, one of the moſt eminent ſurgeons of his time.

After continuing in the line of medical education in London for ſome years, he entered on the practice of his profeſſion, by becoming a military ſurgeon. In the year 1745, he was appointed ſurgeon to Lord Semple's regiment; and he continued in the ſervice of his country, with the armies, both in Scotland and Flanders, till the peace in 1748. Upon the return of the troops to Britain, and the reduction of the regiment to which he belonged, he ſettled as a medical practitioner, and obtained the degree of Doctor of Medicine from the Mariſchal College of Aberdeen.

After having practiſed at Penrith for two years, he accepted of the ſurgeoncy of the 33d regiment, then at Minorca; in which iſland he remained with his regiment for three years. After the return of the regiment to Britain, in 1753, they came to Edinburgh; and an opportunity being thus afforded to Dr Huck of attending the lectures at that medical ſchool, he became the pupil of all the profeſſors, whoſe ſucceſsful exertions firſt raiſed the reputation of Edinburgh as a ſeminary for medical knowledge. During the courſe of two ſucceſſive winter-ſeſſions, he attended the different lectures at Edinburgh with as great aſſiduity as if he had been but entering on the ſtudy of his profeſſion.

He returned with his regiment to England in 1755; and ſoon afterwards he went out to America under the Earl of Loudoun, who was then appointed Commander in chief, and Governor-general, and to whom his medical abilities were well known during the war in Flanders. He was now promoted to the rank of phyſician to the army, and ſerved in that capacity during the whole war, much to his own credit, and to the benefit of the troops under his care. In the end of 1762, he returned with the army to Britain, from the ſucceſsful expedition againſt the Havannah, but in a ſtate of very bad health. Being adviſed to viſit the continent for the re-eſtabliſhment of his conſtitution, he reſolved on making a medical tour through Europe. During this tour, he viſited the moſt celebrated hoſpitals in Germany, Italy, and France: and he embraced every opportunity of making an accurate and candid ſurvey of whatever he could diſcover to be new and uſeful in the practice of phyſic, to the improvement of which he was entirely devoted. Theſe obſervations were immediately communicated to the late Sir John Pringle, who had, for a long time, been the friend and patron of his merit, and who, in his obſervations on the army diſeaſes, has oftener than once mentioned him in a way very much to his honour. Fourteen long letters, which Dr Huck wrote upon theſe ſubjects, and which are now in the poſſeſſion of the learned and worthy phyſician, to whom we are principally indebted for this account of him, give, in the opinion of every able judge who has peruſed them, a more juſt, authentic, and judicious eſtimate of the ſtate of medical practice in the hoſpitals of Vienna, Boulognia, Rome, Naples, Lyons, and Paris, than is any where to be found.

After his return from the continent, he ſettled as phyſician in London, and practiſed there for many years, with great reputation and ſucceſs. In 1765, he was admitted a licentiate of the College of Phyſicians in London; and very ſoon afterwards he was appointed phyſician to the Middleſex Hoſpital. A few years after this, he was appointed phyſician to St Thomas's Hoſpital. And he diſcharged the duties of his office in both, with great credit to himſelf, and advantage to theſe charitable eſtabliſhments. But while he was much eſteemed as a phyſician, he was alſo well known as a philoſopher. He was a diſtinguiſhed and uſeful member of the Royal Society of London, and, for many years, held the important office of being one of their council.

In 1777, he married the niece and heireſs of the late Sir Charles Saunders, to whom, as a naval commander, his country had been much indebted. By this marriage he became poſſeſſed of a large fortune, both in land and money; and it became neceſſary for him to uſe the name and arms of Saunders. After bringing him two daughters, who now inherit her fortune, ſhe died in 1780. His health, which had been every winter much affected with pulmonic complaints, now became worſe, and his ſpirits never recovered the ſhock they received by Mrs Saunders's long illneſs and death.

Although his practice was, for the laſt years of his life, often interrupted by his illneſs, yet he never relinquiſhed it entirely. And his reputation as a phyſician continued to increaſe in the eſteem of mankind in general, and of his medical friends in particular, till his death, which happened on the 24th of July 1785, to the inexpreſſible regret of all who were acquainted with his character.

Much might be ſaid of him as a man, and as a phyſician, did not his long eſtabliſhed reputation for humanity, candour, diſintereſtedneſs, and medical knowledge, render every attempt of this kind unneceſſary. Theſe have exalted his character, in the eſteem of mankind, beyond any panegyric. We may only conclude with obſerving, that the College of Phyſicians in London, as a proof of the opinion which they entertained of Dr Saunders, voluntarily admitted him one of their Fellows; an honour which has hitherto been almoſt entirely confined to graduates of the Univerſities of Oxford and Cambridge. While this diſtinguiſhing mark of approbation, conferred upon Dr Saunders, in the laſt year of his life, in conjunction with another moſt reſpectable medical character, Dr Watſon, was highly honourable to theſe gentlemen, there can be little doubt, that, in the opinion of the public in general, the College did honour to themſelves by this proof of their liberality and candour.

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On the 1ſt of October 1785, Dr Charles Collignon, Profeſſor of Anatomy in the Univerſity of Cambridge, died in that city. Dr Collignon was born in London on the 30th of January 1725. He was the ſon of Mr Paul Collignon, a native of Heſſe Caſſell, and miniſter of the Dutch church in Auſtin Friars, a man univerſally eſteemed for his learning and piety, as well as for the faithful diſcharge of his paſtoral office. From this, however, he had not an opportunity of deriving theſe advantages which might have been at firſt expected; for the Reverend Mr Collignon died at an early period of life, leaving his only ſon yet in an infant ſtate. By thoſe who ſuperintended his education, he was fixed at the ſchool of Bury, under the Reverend Mr Kynneſman, an able teacher.

After the uſual courſe of ſchool education, he was admitted penſioner of Trinity College, Cambridge, in 1743. Having fixed upon medicine as the profeſſion which he meant to follow,

low, after reſiding for ſome time at Cambridge, he viſited France and Holland, and ſpent ſome time at Leyden and at London in the proſecution of his ſtudies. But the academical part of his medical education was chiefly conducted at Edinburgh, where he not only attended to the lectures of the different profeſſors with great aſſiduity, but gave many proofs of his own genius, by making a conſpicuous figure in the Medical Society, of which he was admitted a member in 1747. And by his conduct there he gained the affection, as well as eſteem, of his fellow ſtudents.

Upon his return to Cambridge in 1748, he obtained the degree of Bachelor of Phyſic, from the Univerſity there, and ſettled as a practitioner in that city. He married in 1751, and was elected Profeſſor of Anatomy in Cambridge in 1753. In diſcharging the duties of this office, he gave public lectures till the year before his death. Theſe, though not conducted upon a very extenſive plan, were much eſteemed by all his pupils.

But the anatomical chair was not the only academical honour conferred upon him. In 1754, he was created Doctor of Phyſic, and in

1770, he was elected a Fellow of the Royal Society of London. In 1779, he was appointed deputy Regius Profeſſor of Phyſic at Cambridge; and in 1783, he was appointed Profeſſor of Medicine in Downing College. His conduct and character through life as a man and a phyſician, were ſuch as gave abundant ſanction for thoſe numerous marks of honourable diſtinction conferred upon him by able judges of literary and medical abilities.

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On the 10th of November 1785, died at Preſtonfield, in the county of Mid Lothian, Sir Alexander Dick, Baronet, the ſenior member of the Royal College of Phyſicians in Edinburgh, in the 83d year of his age, a man who, during the whole courſe of a long life, ſupported a character which would have done honour to any learned ſociety.

He was born on the 23d of October 1703. He was the third ſon of Sir William Cuningham of Caprington, Baronet, by Dame Janet Dick, only ſurviving child and heireſs of Sir James Dick of Preſtonfield, Baronet. Having made choice of medicine as the profeſſion he meant to follow, his academical ſtudies in that ſcience were chiefly conducted at Leyden, under the celebrated Dr Boerhaave, then the moſt eminent medical profeſſor in Europe, of whom he was a great admirer. He obtained the degree of Doctor of Medicine, from the Univerſity of Leyden, on the 31ſt of Auguſt 1725. Soon after this he returned to his native country, and had the honour of receiving a ſecond diploma for the degree of Doctor of Medicine, which was conferred upon him by the Univerſity of St Andrew's, on the 23d of January 1727. A few days after this, he was inrolled in the liſt of the College of Phyſicians in Edinburgh, and was raiſed to the rank of a Fellow in that Society on the 7th of November following.

After being honoured with theſe marks of diſtinction in his native country, which are rarely conferred on young men, unleſs of conſpicuous merit, he made the tour of Europe, and reſided a conſiderable time in Italy, where his elegant taſte and claſſical knowledge could not fail to afford him a high degree of gratification. On his return to Britain, he was induced to ſettle as phyſician in Pembrokeſhire, by his friend Mr Hooke, who inherited an ample fortune in that county. He there practiſed medicine with great reputation, and much ſucceſs, for ſeveral years. And during this period, a circumſtance occurred, which ſtrongly marked his liberal mind. A gentleman who had no near relations, and who conſidered himſelf as having been repeatedly indebted to Sir Alexander for the preſervation of his life, intended to have made him his heir, and had actually executed a deed for that purpoſe. But not long after this, by the death of his immediate elder brother, Sir William Dick, he ſucceeded, agreeable to the entail and patent, to the eſtate and honours of Preſtonfield. Upon this event, he repreſented to the gentleman who intended to have made him his heir, that he was now poſſeſſed of a very ample fortune, and he perſuaded him, to ſubſtitute in his place a deſerving young man, to whom that fortune, in conſequence of this advice, devolved in a ſhort time after.

Sir Alexander, upon his brother's death, fixed his reſidence at the family ſeat of Preſtonfield, which is little more than a mile from Edinburgh. And although he entirely relinquiſhed the practice of medicine, yet he cultivated a friendly and intimate correſpondence with the phyſicians of that city. At the annual election of the College of Phyſicians in the year 1756, he was choſen their Preſident. To this office he was re-elected for ſeven years ſucceſſively. But he then poſitively declined accepting of it any longer, as he conſidered it to be an injury to the merits of other gentlemen, that there ſhould not be ſome rotation. His attachment to the College, however, and his earneſt endeavours to promote its intereſt, continued without any abatement. No man was more liberal in contributing towards the building of their hall, and in uſing his beſt endeavours to forward that and every other undertaking, where he thought their intereſt concerned.

But the College of Phyſicians were by no means the only ſet of men who were indebted to his exertions; for he poſſeſſed public ſpirit in the higheſt degree; and on all occaſions he demonſtrated the ſtrongeſt deſire to promote what was beneficial to his country in general, and to the city of Edinburgh in particular.

There is no perſon to whom the public is more indebted for the many excellent roads which have lately been made in the neighbourhood of Edinburgh; and moſt of the internal improvements of the city, he exerted himſelf in promoting, with an activity which did him the higheſt honour.

When the ſeeds of the true Rhubarb were firſt introduced into Britain by Dr Mounſey, he not only beſtowed great attention on the culture of the plant, but alſo on the drying of the root, and preparing it for the market. And his ſucceſs in theſe particulars was ſo great, that the Society in London, for the encouragement of arts and manufactures, ſent him a golden medal, as a teſtimony of their approbation. Several years before his death, as a teſtimony of eſteem and gratitude for his ſervices, a picture of him was placed in the library of the College of Phyſicians in Edinburgh; and that body, at their firſt meeting after his death, were all dreſſed in mourning, a ſlight external mark of their feelings for the loſs which their Society had ſuſtained by the death of ſuch a member. For although he had arrived at a period of life, when death is, with the majority

jority of mankind, perhaps rather to be wiſhed for than otherwiſe; yet, as not only his judgment, but even his ſpirit of exertion remained unimpaired, both they and the public were, by this event, deprived of an uſeful member of ſociety.

Whatever object engaged his attention, he was ſteady in the purſuit; and his conduct was always marked by the ſtricteſt fairneſs and integrity. This diſpoſition led him to be conſtant and warm in his friendſhip. And this conduct procured to him univerſal love and eſteem. But he was not more amiable in public, than in private life. For with all his diſpoſition for activity and exertion, the ſtriking features of his character were mildneſs and ſweetneſs of temper. He poſſeſſed the happy diſpoſition of viewing the fair ſide of every object, which was not only the ſource of much happineſs to himſelf and his family, but of univerſal benevolence to mankind. The ſerenity and cheerfulneſs which accompanied his conduct through life, were the attendants even of his laſt moments, for he died in the eaſieſt way, and with a ſmile upon his countenance.

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We mentioned in our laſt volume, that the Harveian Society of Edinburgh, had propoſed, as the ſubject of their prize-diſſertation for the year 1784, an experimental enquiry concerning the nature and properties of ipecacuanha. This ſubject of inveſtigation gave riſe to a very intereſting competition; but the judges were unanimous in conſidering, as the firſt in point of merit, a diſſertation, to which was prefixed the following motto:

Fave, Phœbe,
Novus ingreditur tua templa ſacerdos.

And upon opening the ſealed letter which accompanied it, they found they were indebted, for this diſſertation, to Dr Ralph Irving from Langholm, the gentleman who had obtained their firſt prize for 1783, on the ſubject of the Peruvian bark. As it is not improbable that the ingenious author may publiſh this diſſertation, as he has done his treatiſe on the Peruvian bark, the public will then have an opportunity of judging of its merits. We ſhall here, however, preſent our readers with a ſhort view of ſome important particulars contained in this eſſay.

It has long been known, that the root of ipecacuanha is impregnated both with a gummy and reſinous matter, blended through its whole ſubſtance. But Dr Irving has aſcertained, that the gummy exiſts in a much greater proportion than the reſinous part. Accordingly cold infuſions are not only ſo thick as to be unable to paſs through paper, but ſuffer no precipitation by alkalis or lime-water.

That the root of ipecacuanha conſiſts of a cortical and ligneous part, muſt obviouſly appear to every one who takes the trouble of breaking it. And it has long been ſuppoſed, that the former is more powerful than the latter. This is now aſcertained beyond doubt; and it has been farther proved, that as the cortical part yields firſt to the peſtle, the powder firſt obtained is the ſtrongeſt. Still, however, even the pure ligneous part poſſeſſes a very conſiderable degree of emetic power.

Though a reſinous part is evidently contained in ipecacuanha, and although this reſin, obtained in a ſeparate ſtate, has been found to poſſeſs an emetic power, yet from accurate trials, made by repeated affuſions of rectified ſpirit, it has been found, that this reſinous part is by no means conſiderable, and that it is not, as ſome have ſuppoſed, the foundation of the emetic quality: for the reſiduum of theſe infuſions was found, even in ſmall doſes, to produce vomiting.

Ipecacuanha has long been conſidered as poſſeſſing ſome degree of antiſeptic power. But from Dr Irving's experiments, demonſtration is afforded, that it poſſeſſes this power to a very conſiderable extent.

An aſtringent power in ipecacuanha, was formerly rather ſuppoſed than proved: now, however, from the formation of ink, by means of this vegetable, from the teſt of chalybeates, and other marks, it appears, not only that its aſtringent power is conſiderable, but that this aſtringency may be extracted by a ſpirituous menſtruum, and that it has probably no connection with its emetic quality.

Some have ſuppoſed the emetic power of ipecacuanha to be of a volatile, others of a fixed nature. This has led our ingenious author to make a compariſon of the effects of the water obtained from it by diſtillation, with the decoction which remained in the ſtill, after the diſtillation had been performed. And while it was found, that the former had very little influence, the latter not only operated violently as an emetic, but produced other ſymptoms, even of an alarming nature. Although, however, its qualities are thus powerfully extracted by decoction, yet it was alſo found, that, by very long continued boiling, the emetic power of this root is almoſt totally deſtroyed.

The emetic power of ipecacuanha, however, appear from Dr Irving's experiments, to be much more readily counteracted in another way, by the influence, *viz.* of the acetous acid. As the reſult of his trials upon this ſubject, appears to be no leſs important than new, we ſhall here preſent our readers with the detail of one of his experiments in his own words.

Experiment 41. To a man, fifty years of age, I gave (ſays he) thirty grains of ipecacuanha, mixed with three ounces of diſtilled vinegar. After ſitting by him for two hours, and finding that he continued free from even the ſlighteſt nauſea, I allowed him to go about his uſual occupation. Next day he informed me that he had two or three looſe ſtools during the night, but without gripes or other uneaſineſs.

If this effect of the acetous acid, in counteracting the emetic power of ipecacuanha, ſhall be confirmed by future obſervations, the diſcovery will be of conſiderable importance in the practice of medicine.

After Dr Duncan, one of the Secretaries of the Harveian Society, had delivered the prize-medal to Dr Irving, he announced, as the ſubject of the prize-queſtion for next year, An experimental inveſtigation of the nature and properties of opium, of its different conſtituent parts, and of their effects on the human ſyſtem.

In recommending this as a ſubject of great importance, he obſerved, that, without travelling back to the encomiums beſtowed upon it by the ancients, it was only neceſſary to ſubmit, to the conſideration of his hearers, the following character of it, in the words of the ſagacious Sydenham. That truly intelligent and diſcerning phyſician obſerves, "Ita neceſſarium eſt opium in hominis periti manu, ut ſine illo, manca ſit et claudicet medicinā, qui vero eodem inſtructus fuerit, majora præſtabit, quam quis ab uno remedio facile ſperaverit." Although it was hardly poſſible to go farther in the praiſes of any article, yet, for any thing hitherto diſcovered, even this character is not beyond the truth. But notwithſtanding all that has been aſcertained reſpecting the powers of opium, many doubts and deſiderata ſtill remain. In many inſtances, where it is at firſt followed by the moſt ſalutary effects, it is afterwards productive of diſagreeable conſequences. And it yet remains to be determined, by accurate experiment, whether its ſoporific and intoxicating effects depend upon the ſame, or on different active principles which enter its compoſition. Hence, there can be no doubt that it diſplays a field from which laurels may be reaped. And he who ſhall diſcover the moſt certain and eaſy method of correcting the virulent effects of opium, will have a juſt claim to the thanks of his cotemporaries, and to the gratitude of poſterity. Nay, he who makes an induſtrious attempt towards this inveſtigation, by faithful and accurate experiment, although his reſearches ſhould prove unſucceſsful, will yet receive an ample compenſation for his labours in that heartfelt ſatisfaction which is the natural and neceſſary conſequence of well meant and induſtrious exertion.

At the annual meeting of the Harveian Society on the 12th of April 1785, prior to the delivery of the prize-medal, Dr Webſter, one of the Secretaries, delivered the anniverſary oration, the ſubject of which was an account of the life, writings, and character, of the late Sir John Pringle, Baronet.

The diſſertations on the ſubject of opium, which have been tranſmitted to the Secretaries, are now under the conſideration of the Society; and the prize-medal will be preſented to the ſucceſsful candidate on the 12th of April 1786, after the delivery of the Harveian oration for the preſent year.

At that time, the prize-queſtions for two ſucceeding years will be announced, and the different particulars which the Society have had principally in view in propoſing them will be explained. We may, however, obſerve, that the prize-queſtion propoſed for the year 1786, is, An experimental enquiry concerning the nature and properties of the hyoſcyamus niger of Linnæus; its effects on the human ſyſtem; its uſe in the cure of diſeaſes; and the comparative power of different parts of the plant?

The prize queſtion for 1787, is, An experimental enquiry concerning the chemical and medical effects of thoſe ſubſtances called lithontriptics particularly on the human calculus?

Diſſertations on the firſt of theſe ſubjects muſt be tranſmitted to Drs Duncan or Webſter by the 1ſt of January 1787; and, on the laſt, by the 1ſt of January 1788. Each diſſertation muſt be accompanied by a ſealed letter, bearing the ſame motto with the diſſertation, and incloſing the name of the author.

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The Royal Medical Society of Edinburgh have publiſhed the following advertiſement reſpecting their prize-medal for 1786.

" Omnibus ad quos hæc pervenerint, ſalutem. Quoniam ſatis conſtat, commoda magna quidem et plurima ex præmiis et honoribus publicè propoſitis redundâſſe, Societas Regia Medica Edinenſis, ſymbolum quoque ſuum ad ſcientiam promovendam conferre decrevit, et comitiis ſolenni more ad id habitis, quæſtionem ſequentem proponere, et auctorem diſſertationis

præſtantiſſimæ, aureo numiſmate 21 libris valente, condecorare, ſtatuit; ſcilicet, Quot ſint fermentationis ſpecies, quænam cujuſque natura, nec non ex quibus corporum conditionibus, zymica inter et antizymica differentia pendeat?

"Hujus inſtituti hæ ſunt conditiones: 1mo, Diſſertationem ſuam Latinè conſcriptam mittendam curabit auctor, ad eos qui a ſecretis fuerint ad acta edenda, apud ædes Societatis Edinburgi, in diem Januarii primam, anno 1787. 2do, Epiſtola inſuper ab auctore mittenda eſt, nomen ſuum locumque quo habitat indicans, eodemque ſigillo ac ipſa diſſertatio munita, nota qualibet parti ſuperaddita exteriori, quæ alteram diſſertationi præfixam referat. Niſi autem præſtantiſſima et præmio digniſſima dijudicata fuerit diſſertatio, ad eum quocunque placuerit, remittetur, una cum epiſtola, intacto ſigillo. Vel ſi de hoc parum ſolicitus ſit, ambæ combuſtæ dabuntur. 3tio, Die menſis Aprilis prima ejuſdem anni, diſſertationi optimæ præmium adjudicabitur, quam ſub quavis forma cunque in lucem, edendi penes Societatem jus eſſe ſemper intelligendum.

" *N. B.* Poſt diem primam Januarii proximi, nullam accipiet diſſertationem Societas de quæſtione quam anno jam propoſuit elapſo, videlicet, Quot ſint aëris ſpecies, quænam ſingularum natura, et in medicina vires? Præmium vero Aprili ſequente adjudicabitur.

Jac. Jeffray, Tho. Ad. Emmet, M. D. Jac. M'Donnell, M. D. Tho. Skeete, M. D. Præſides annui.

Gul. Cullen, M. D. Jo. Black, M. D. Ja. Gregory, M. D. e Col. Reg. Med. Edin.

B. Bell, Tho. Hay, Jac. Ruſſell, e Col. Reg. Chir. Edin.

Jo. Wilſon, G. P. Hayle, G. Young, G. Alexander, e Soc. ordinariis.

A. Duncan, M. D. Ca. Stewart, M. D. ab epiſtolis, ad acta Societatis edenda."

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The Faculty of Medicine at Goettingen have propoſed the following prize-queſtion for the year 1786.

" In irritabilitate animali, a qua niſi omnis, ſaltem maxima ſane pars principii in corpore noſtro actuoſi pendet, varia ſuperſunt quæ pleniorem poſcunt eruditorum expoſitionem, de-

ſiderat ergo ſolidam, experimentis et obſervationibus ſuperſtructam, reſponſionem, ad varias quæſtiones : 1mo, In quanam elementorum corporis animalis parte hæret vis irritabilis ? 2do, Qui ſunt veri certique fines, inter tonum Stahlii et irritabilitatem Halleri ? 3tio, Quibus adminiculis, ſive a climate, aëre, et vitæ genere, ſive a cibo potuque, ſive a medicamento, deſumptis effici poteſt, ut ſi torpida languet, aut plane deficit, excitetur et augeatur ? vel ſi nimis efficere ſit, temperetur et coërceatur ? 4to, Quale invicem obſervatur commercium irritabilitatem inter et nervorum ſyſtema, in doctrina affectuum animi, temperamentorum, et in genere principii actuoſi corporis animalis, ullo cum fructu adhibendum ?”

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The Academy of Sciences at Berlin have propoſed the following prize-queſtion for the year 1786.

“ Etablir par des expériences exactes, décrites à clarté et préciſion, la théorie de la fermentation, et de la compoſition quelle fait éprouver aux corps qui la ſubiſſent, et de la

nouvelle compoſition des principes, qui en reſultent dans ſes differens périodes?"

The Academy wiſhes that experiments ſhould be inſtituted not only upon unorganized bodies, but that attention ſhould alſo be paid to that fermentation which takes place both in vegetables and animals, particularly that by which thoſe matters which ſerve for the vegetation of plants, and the nouriſhment of animals, become fit aliment for them, and enter into the compoſition of their ſolids and fluids. The learned of all nations are invited to the competition for this queſtion; and the value of the prize is L. 50.

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In 1775, the Royal Academy of Sciences at Paris propoſed the following ſubject for prize-diſſertations:

"Des recherches ſur la meilleure méthode de conſtruire, et de ſuſpendre les aiguilles aimantées, et de s'aſſurer ſi elles ſont, dans le plan du méridien magnétique, de manière qu'on puiſſe obſerver avec ces aiguilles, les variations diurnes de la déclinaiſon."

Nothing of ſufficient importance being at firſt given in, on this ſubject, it was repeated for a future year, and has now been divided between two diſſertations; the one having for its device, " Etiam non aſſecutis voluiſſe abunde pulcherum atque magnificum eſt," which was found to be written by Mr Van-Swinden, Profeſſor of Philoſophy at Franequer in Frizeland; the other, having for its device, " Facilius quid non ſit, quam quid ſit de hujuſmodi rebus poſſe confirmari," was found to be written by M. Coulomb, Captain of the Corps Royal de Génie.

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A company of Merchants, diſtinguiſhed by their enlightened zeal for the progreſs of arts, having ſent to the Royal Academy of Sciences in Paris the ſum of twelve hundred livres for prizes on the art of dyeing, the Academy accepted of their offer with thoſe acknowledgments which they owe to all who concur with them in their endeavours to diffuſe and extend uſeful knowledge. And they accordingly propoſed the following ſubject for a prize-diſſertation: " L'analyſe de l'indigo, et l'examen chymique des opérations employées dans les teintures,

tures, dont cette ſubſtance eſt la baſe." Many diſſertations were preſented to the Academy, containing ingenious experiments and uſeful views; but the Academy reſolved to divide the prize between two of theſe; the firſt having for its device,

"Vincant queis Neptune dediſti
Quamquam ô,——"

written by Mr Quatremère;
the ſecond having for its device,

"Felix qui potuit rerum cognoſcere cauſas."

The theoretical part of this diſſertation was written by M. Hecquet d'Orval, and the experimental by M. de Ribaucourt.

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Though many regiſters of the weather have been kept with tolerable exactneſs in Edinburgh, and its environs, yet for ſeveral years paſt, none of theſe have been publiſhed. It is with pleaſure we can now preſent our readers with the general reſult of obſervations, made with great accuracy, during the courſe of laſt year, on the ſtate of the thermometer and barometer, and the quantity of the rain about a mile from Edinburgh.

	THERMOMETER.			BAROMETER.			RAIN.
	High.	Low.	Med.	High.	Loweſt	Med.	
January,	48	27	37.7	30.18	28.87	29.92	1.50
February,	39	20	29.9	30.85	29.00	29.49	2.31
March,	42	18	30.0	30.47	29.80	30.17	0.43
April,	68	37	57.6	30.53	29.63	30.01	0.44
May,	70	47	59.8	30.86	29.00	29.98	0.94
June,	89	43	68.0	30.50	29.78	30.16	1.11
July,	83	59	70.0	30.80	28.96	29.98	1.44
Auguſt,	67	54	61.8	30.21	29.34	29.97	3.24
September,	70	48	63.0	30.42	29.00	29.42	10.72
October,	65	28	50.0	30.48	29.30	30.20	2.46
November,	50	29	40.0	30.52	29.55	30.34	4.71
December,	45	23	34.6	30.39	29.25	29.97	1.33

Total rain, 30.63

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By the following extract of a letter from the Iſle of France, in the neighbourhood of Madagaſcar, written by Mr Ceré, ſuperintendant of the trees in the royal garden eſtabliſhed there, to the late Mr Bertin ſecretary of ſtate in France, it appears, that the culture of ſome of the moſt valuable ſpiceries is there in ſuch a ſtate, as to give the moſt flattering hopes of future ſucceſs; and there is reaſon to believe, that, by proper attention, cloves and nutmegs might be cultivated in other climates.

“ Le jardin du roi eſt toujours dans le meilleur état poſſible, et les girofliers ſe multiplient de manière à donner les plus grandes éſpérances.

ces. Nous n'avons que dix muſcadiers femelles de reconnus, dont ſix en rapport: un d'eux a plus de cinquante fruits. C'eſt ſeulement dommage, qu'ils ſoient tous deſtinés à ne donner que des muſcadiers mâles, auſſi je deſtine ces premières noix muſcades Françoiſes à vous, Monſeigneur, le protecteur conſtant de ces arbres precieux, à Monſieur Poivre leur introducteur, et à Monſieur de Sartine, pour la confiance qu'il a bien voulu prendre dans mes ſoins; pourque vous puiſſiés en faire la comparaiſon, ainſi que du macis, avec cette mince épicerie Hollandoiſe. Les derniers vaiſſeaux partis vous portent, Monſeigneur, mes nouvelles remarques ſur cet arbre très extraordinaire, dans la façon de ſe reproduire: je ſouhaite que vous les trouviés dignes de prendre un moment de votre tems."

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A phyſician of very great eminence in England, in a letter lately written to Dr Duncan, has communicated to him the following particulars reſpecting a caſe of tympanites, and a caſe of hydrocephalus.

"I had lately under my care, a caſe of tympanites. The body was ſubmitted to inſpection after death. About the middle of the rectum was a thickening and contraction of that gut, ſo as to cloſe up the paſſage. But during life there was no ſymptom or complaint which could lead to the ſuſpicion of that being the ſeat or cauſe of the diſeaſe. The patient was a female about forty-ſix years old. The ſenſe of diſtenſion came on gradually, nor was there to the laſt any febrile diſpoſition. There was found much air in the cavity of the abdomen, which had eſcaped from a rupture at the flexure of the colon.

"I can now lend my teſtimony to confirm the good effects of mercury in hydrocephalus. A boy ſeven years old, appeared to me to labour under the ſymptoms of that diſeaſe. The free uſe of calomel and unguentum mercuriale recovered him. In five or ſix weeks he relapſed, and was again cured by the ſame means. After two months of apparently good health, when the ſymptoms appeared to be gone, and whilſt he was mending apace from the mercurial treatment, he was ſeized by the meaſles. Soon after the vaniſhing of this eruption, he complained more than ever of his head, was convulſed, and died. The ventricles contained near four ounces of watery fluid. The medullary ſubſtance of the brain had a fluid pulpy conſiſtence; and about half an ounce of blood was extravaſated and coagulated."

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We have lately been favoured with two caſes of inſanity, one of which was cured by the uſe of fox-glove; and alſo a caſe of hæmoptyſis cured by the ſame remedy. Theſe were treated by Mr William Jones, ſurgeon at Birmingham, and will be publiſhed in the next volume of this work. We are, however, ſorry to obſerve, that this remedy has not been attended with the wiſhed for effects, in the hands of every practitioner who has employed it. During the courſe of this winter, it has been uſed in ſeveral dropſical caſes in the Royal Infirmary, and from its failing to produce the neceſſary diſcharge of water, recourſe was obliged to be had to other remedies. In the caſe of one woman, to whom it was directed in doſes of two grains of the powder of the leaves

thrice

thrice a-day; after it had been taken for two days very violent vomiting enſued, which notwithſtanding the employment of various remedies, continued for the ſpace of ſix days, in ſuch a manner, that nothing could be retained on her ſtomach. And on the morning of the ſeventh day, from the commencement of the vomiting, ſhe died. It was the opinion of many of the ſtudents, that this vomiting was excited by the digitalis.

But from the following account of the appearances which were diſcovered on diſſection, an explanation is not only afforded for the obſtinacy of the diſeaſe; but it is alſo rendered probable, that the ſevere vomiting was to be explained in another manner than from the action of the digitalis.

The body was opened by Mr Fyffe, diſſector to Dr Monro. The abdomen was ſo diſtended, that he was induced to draw off part of the fluid contained in it, by the trocar, before making any inciſion through the integuments. Upon cutting through them and the abdominal muſcles, he was ſurpriſed to find, as he thought, the peritonæum much thickened and ulcerated. After, however, the greater part of the fluid was diſcharged, the thickened membrane which attracted his attention, was found to be a cyſt, filling completely the cavity of the abdomen, and connected by cellular ſubſtance to the peritonæum.

Several ragged irregular tumours, ſome of which were larger than a pigeon's egg, were found adhering to its internal ſurface, which was alſo ragged and ulcerated in many places. From theſe, when cut, a limpid gelatinous fluid, reſembling the white of an egg iſſued out, though the contents of ſome of them reſembled the vitreous humour of the eye. The bag was found, on more narrow inſpection, to be connected with the uterus, and to be the left ovarium. Towards its fundus, where it joined the uterus, there was a large tumour, irregular in its internal appearance, but ſmooth in its outer ſurface, nearly the ſize of a child's head. When cut, it was found to contain a quantity of the ſame fluid with the hydatides above mentioned, together with a number of ſimilar hydatides, filled in the ſame manner.

The ſpermatic veſſels on the left ſide were much enlarged. The left ſide of the fundus uteri was lengthened out, and drawn upwards.

The uterus upon being cut, was found to be of a hard cartilaginous texture. The alimentary canal was found to be lodged behind the cyſt; and about half a foot of the lower end of the inteſtinum ileum, was found in a gangrenous ſtate, and, in one part of it, the ſides of the inteſtine, were nearly completely coaleſced. Some inflammation and ſlight adheſions were found along the tract of the ileum. The liver was rather pale and ſmall; the ſtomach was in its natural ſtate; the other viſcera were ſound; but the ſpleen was ſmaller than common. The quantity of gelatinous fluid mixed with pus, found in the ſac and hydatides, was computed to amount to about twenty-ſix pints.

While the obſtinacy of this caſe is readily explained from the gelatinous nature of the fluid, as well as from the ſacs and hydatides in which it was contained; when it is conſidered that the ſtomach was in a perfectly ſound ſtate; and when regard is paid to the highly morbid condition of the inteſtinal canal, at the inferior part of the ileum, it muſt, at leaſt, be allowed to be a matter of doubt, whether the digitalis had any ſhare in inducing the vomiting which here took place.

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A letter from a gentleman who has lately returned from the continent of Europe, gives the following account of the uſe of opium, in the venereal diſeaſe.

"In the military hoſpital at Liſle, I ſaw the moſt ſucceſsful practice in the venereal diſeaſe that I have yet known. The phyſician of that hoſpital has, for ſome time paſt, treated all his patients, labouring under ſyphilis, with opium alone; and has met with the moſt unexpected ſucceſs. He has adminiſtered this medicine to near 500 venereal patients, labouring under the diſeaſe in every form. He begins at firſt by giving it in doſes of half a grain, or a grain; and he gradually increaſes the doſe, till the patient takes twenty, or twenty-five grains in the ſpace of a day. He gives it under the form of pills, and continues augmenting the doſe, till the opium produces ſome conſiderable affection of the ſyſtem, either in the way of vomiting or vertigo."

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The following intelligence reſpecting the effects which ſometimes reſult from tranſplanting teeth, is extracted from a letter to Dr Duncan by one of his medical friends.

"A new diſeaſe has lately been diſcovered in London, occaſioned by the tranſplanting of teeth from the head of one perſon to that of another. The mortality from it is computed at nearly two deaths to ten diſeaſes; and about one in every hundred, who receive teeth by tranſplantation, are affected with the diſeaſe. Ulcerations of the throat and gums, and eruptions on the ſkin, are the chief marks of the diſeaſe. When death takes place, it is from the occurrence of ſphacelus. For five or ſix weeks after tranſplanting, the teeth look well, and are as firm as the others. A cure has been attempted, in vain, by the Peruvian bark; and it has proved fatal after the uſe of mercury, although ſome caſes have yielded to that medicine."

In the third volume of the London Medical Tranſactions, the hiſtory of a caſe of this diſeaſe is recorded by that very reſpectable phyſician Dr Watſon of London: After various attempts for the cure, it terminated fatally.

Dr

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Dr Pearſon of London, who is already well known to the medical and philoſophical world, by his ingenious obſervations and experiments on the tepid ſprings of Buxton, a place at which he, in general, reſides for ſome time every ſummer, has lately printed and diſtributed among his friends, directions for impregnating the Buxton water, with its own and other gaſes, and for compoſing artificial Buxton water. He continues his experiments alſo on the effects of the water with great induſtry; and, among others, he has lately communicated to Dr Duncan the following particulars:

" Among the experiments which I made to aſcertain the power of Buxton water in the cure of diſeaſes, I inſtituted ſome, with a view to determine its effects upon the perſpiration. I found, by repeated trials, that during the time the body was immerſed up to the chin, from half an hour to two hours, there was no perſpiration from the ſkin immerſed; and that this excretion from the lungs and head was the ſame in quantity during the bathing, as when the body is in air of a common temperature. I found alſo, that when the body was thus immerſed, there was no abſorption of water,

ter, although the kidneys were, in this ſituation, conſiderably ſtimulated to excrete urine. Theſe facts I aſcertained by weighing the body in ſcales, nearly of the ſame form with thoſe uſed by grocers, but which were made purpoſely, ſo as to weigh leſs than a quarter of an ounce, when loaded at each end of the beam with one hundred and fifty pounds.

"The concluſion then is, that there is no evaporation from the ſurface of the body, or abſorption, while it is immerſed in water of the temperature of 82°.

"The effect of bathing in the Buxton water, on the action of the ſanguiferous ſyſtem, is to diminiſh the frequency of the pulſations of the heart and arteries, and to contract the arteries. For after the body had been immerſed ten minutes, I uniformly found the pulſe at the wriſt leſs frequent, and ſmaller than before entering the bath; and, in general, it diminiſhed in frequency and ſize for half an hour longer. But after this, the heart maintained the ſame number of pulſations in a given time, and the arteries did not contract in their dimenſions farther.

"There have been great miſtakes, I am ſure, on the inferences from experiments made to aſcertain the effects of bathing on the pulſe. If attention be not paid, the effects of motion, &c. will be confounded with thoſe of the bath. I was in an error for ſome time on this account. At laſt I always kept the body at reſt at the bath-ſide, or room adjoining, and entered the bath without moving the body conſiderably. Having entered the bath, I ſat or ſtood there up to the chin, and in that ſituation I made my obſervations. The degree of diminution in the number of pulſations, and in the ſize of the arteries, depended on the ſtate of the body at the time. If by food and wine the pulſe was increaſed in frequency, as, for example, to be 120 in the minute, and full, although the body was at reſt in cool air, it was diminiſhed in half an hour or an hour in the bath, to 80, or even 70, and it became ſmall and hard.

" I ſhall only explain this matter by two examples farther. If the pulſe was firſt examined while the body was at reſt, before bathing, and again examined after plunging, with ſome force, into the bath, or even immerſing the head two or three times, it was found to be increaſed in number for a ſhort time. If after the body had been for ſome time immerſed, and as little motion uſed as poſſible, before and on immerſion; when the pulſe was diminiſhed in ſize and number as uſual, I ſay, if, in this ſtate, the body was moved by merely walking ſlowly in the bath, the number of beats would be increaſed. But it might again be made to diminiſh, by ſtanding or ſitting a ſufficient time longer in the bath. Want of ſufficient attention to theſe effects of motion, has often occaſioned erroneous concluſions.

" The heat of the ſkin, when the body had been of an agreeable temperature, was diminiſhed a few degrees of the thermometer, for ſome time after bathing, and the body in general felt cooler. When the ſkin had been hot ſo as to raiſe the thermometer to 110° or more, it was diminſhed to 80°, or even below it, by bathing. But I did not find this variation of temperature to extend deeper than the ſkin. For after the body had been two hours in the bath, and felt cold, the temperature of the mouth and throat, and of the urethra, was the ſame as before immerſion. I uſed Ramſden's ſmall thermometer, made purpoſely for taking the heat of animals, as deſcribed in Mr Hunter and Dr Fordyce's experiments on the heat of animals, in the Philoſophical Tranſactions."

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Mr Macnab, ſurgeon at Fort Albany in Hudſon's Bay, in a letter to Dr Duncan, informs him, that, in conſequence of directions from the Royal Society of London, he had performed, with very accurate inſtruments ſent him for that purpoſe, different ſets of experiments, to determine the power of the vitriolic acid in producing artificial cold; and that from theſe he had found it much more powerful in producing artificial cold than the nitrous acid. Theſe experiments will probably be publiſhed at full length in the Philoſophical Tranſactions. We may only obſerve, that when the thermometer in the air ſtood at 40 degrees below 0, by the aid of vitriolic acid and ſnow, it was made to ſink to 69 degrees below 0. This is conſiderably lower than the thermometer fell in any of Mr Hutchin's experiments performed at Hudſon's Bay in 1782.

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Dr Matthew Guthrie, phyſician at St Peterſburgh, has lately publiſhed a ſhort treatiſe, which is entitled, "Nouvelles Experiences pour ſervir à déterminer le vrai point de congélation du mercure, et la différence que le degré de pureté de ce metal pourroit y apporter."

In theſe experiments Dr Guthrie had the happineſs of being aſſiſted by Meſſrs Pallas, Coxe, and Hill; and the following is given as the reſult of their obſervations, the ſucceſs of which was by no means equivocal.

1ſt, That the point of cold neceſſary for the congelation of mercury is 32 degrees below 0, in Mr Reaumur's ſcale.

2dly, That mercury in its ordinary ſtate, and even that which is ſurcharged with foreign metallic matters, does not congeal at a leſs degree of cold than the moſt pure revivified mercury, or that which has been treated with alkali; but that mercury purified by antimony congeals at two degrees leſs.

3dly, That by certain circumſtances, the mercury of the thermometer may be cooled ſome degrees below its real point of congelation, without being frozen, whilſt the mercury into which it is immerſed is perfectly in a frozen ſtate.

4thly, That the mercurial thermometer is a juſt meaſure of heat and cold, from the degree of boiling water to the point of the congelation of mercury; but that, below that point, the contraction of the metal renders its progreſs irregular and deceitful.

Dr Guthrie, in a letter to Dr Duncan, gives him the following account of the ſucceſsful treatment of a boy who had been bit by a mad dog.

"A boy, ſervant to my worthy friend Mr Fitzherbert, Britiſh miniſter at this Court, was bit, in the beginning of April, by a dog, which was proved to be mad, as two favourite ſpaniels of his Excellency's both died mad in about a month after being wounded by him on the ſame day on which he bit the boy. I had the youth's wound, on the inſtep of the foot, immediately ſcarified, ſo as to diſcharge a good deal of blood. After this it was dreſſed with ſtrong mercurial ointment, for about fourteen days. And it was kept open by applying occaſionally a ſmall bit of bliſtering plaſter to the part.

"But at the end of the ſecond week, the Ormſkirk medicine being procured from an Engliſh Lady, and Mr Fitzherbert being deſirous of adminiſtering it, as a remedy looked upon as ſpecific in his native county, Derbyſhire, it was given to the boy. The wound after this was dreſſed with the medicine recommended by the inventor for that purpoſe. This continued to keep it open for the remainder of five weeks, during which period he took two courſes of the ſuppoſed ſpecific, in the full proportion ordered by Mr Hill. After the Ormſkirk remedy was begun, Mr Hill's printed directions were religiouſly obſerved, without any interference on my part.

" About the end of ſeven weeks from the accident, and after the bite had been ſolidly healed up for ten or twelve days, and the boy in his wonted health and ſpirits, riding out as formerly to air his Excellency's horſes, he began to complain of ſhooting pains in the cicatrices, for ſeveral days together, and one of them began to inflame, and to put on that appearance which the inciſion of inoculation preſents, before the eruption of the ſmall-pox. I was now convinced that no time was to be loſt, and that no faith was to be put in the infallibility of the Ormſkirk ſpecific. I immediately ordered the cicatrices to be opened, and ſtrong mercurial ointment rubbed on to the quantity of a drachm every morning and evening, until two ounces were conſumed. During this friction, the ſhooting pains and eruptive appearance went off; and the boy now enjoys perfect health and ſpirits, while the ſound ſtate of his foot ſeems to have made him forget his diſmal and melancholy accident.

“But whether it will ever be recalled to his memory by ſuch an attack as is mentioned by ſome phyſicians to have happened long after the inſertion of the poiſon, I will not take upon me to decide. If, however, I may be permitted to offer an opinion in ſuch a doubtful caſe, I ſhould think it will not, but that this, like other contagions acting upon the animal ſyſtem, has a pretty exact and uniform period of producing its effects; and that the vulgar error of confounding that ſpaſmodic diſeaſe reſulting from what is uſually called the bite of a mad dog, with madneſs ſtrictly ſo called, has given riſe to the opinion, that perſons who happen to die with ſymptoms of furiouſneſs, ſeveral years after ſuch accident, have fallen victims to the faithful ſervant of the human ſpecies.

“From this caſe, I would beg leave to draw the following concluſions: 1*ſt*, I think that this is one of a few poſitive examples where the effects of the contagion of rabies have been prevented by medicine. For, from the fate of the two other dogs which were bit by the ſame dog, and on the ſame day, with my patient, there cannot be a doubt that the animal which bit him was actually mad. 2*dly*, From the appearances which took place in the wounds of the boy's foot after they were healed up, and about the period when the diſeaſe commonly ſhews itſelf, we have no reaſon to put more faith in the Ormſkirk medicine than the analyſis of two celebrated chemiſts would naturally lead to, who have found it to be compoſed of calcareous earth, Armenian bole, and ſuch other inefficacious ſubſtances. And, 3*dly*, That the virus is probably not ſubdued merely by the action of the oily matter contained in the mercurial ointment, according to the ingenious ſuggeſtion of Dr Berkenhout, who conceived that idea by reflecting on the effect of oil in counteracting the venom of the viper. For were this the caſe, the application of the ointment to the bite for two weeks ſucceſſively, immediately after the accident, would have had the deſired effect; at leaſt, if we can form any judgment from the analogy which the Doctor ſo happily employs. For in the caſe of bites of poiſonous reptiles, the oil is only uſed for a ſhort time, at the utmoſt, if I remember right, for a few days.

"The real active remedy, then, ſeems to have been the mercury; and that does not ſeem to have ſubdued the contagion, or to have prevented its action, till introduced into the ſyſtem in conſiderable quantity, during the free uſe of it which at laſt took place."

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Dr Guthrie has alſo tranſmitted to Dr Duncan the following account of a curious and valuable maſs of Ruſſian Malachite, which is his property, and which is at preſent in the houſe of Sir Joſeph Banks in London.

"La Malaquite qu'on a ſouvent rangé parmis les pierres demi-précieuſes, à cauſe de ſa beauté, eſt proprement une mine de cuivre, la plus pure peut être qui exiſte, et par cette raiſon la plus rare à trouver en grandes maſſes. Il faut des circonſtances bien favorables pour raſſembler dans les atteliers de la nature des amas un peu conſiderables d'une chaux de cuivre bien pure, et ſans mélange héterogène:

car telle eſt la baſe de la vraie et belle Malaquite, qui ne contient ordinairement que de l'eau et de l'air fixe comme diſſolvants, ſur environ 70 à 80 pour cent de cuivre pur en état de chaux métallique. Auſſi les mines, où la Malaquite ſe trouve en morceaux un peu conſiderables et purs, ſont elles peu frequentes; et quoique la Hongrie, le Tyrol et l'Eſpagne en produiſent dans quelques unes de leurs mines, ce n'eſt pourtant qu'en maſſes très-médiocres, ou en forme d'incruſtation, et pas même de la plus parfaite qualité.

" La Sibérie et la Chine ſont les deux pays les ſeuls célebres pour la belle Malaquite; mais autant que celle de la Chine ſurpaſſe les fauſſes Malaquites de tous les autres pays, tout autant la belle Malaquite de Sibérie lui eſt encore ſuperieure, pour la dureté, la ſolidité et le brillant, auſſi bien que la variecté des couleurs. Je dis la belle Malaquite de la Sibérie; car quoique ce pays en rende en pluſieures mines de la qualité de celles de Hongrie et du Tyrol, il n'y a pourtant que la ſeule mine de Ghoumecheſskoi, exploitée depuis plus de 40 ans, environs à 40 verſtes au ſud de Cathrinenbourg, où l'on tire cette Malaquite dure, fine et fleurie, qui eſt ſi eſtimée par les amateurs d'hiſtoire naturelle, et par les lapidaires. Cette mine, preſqu' épuiſée aujourd'hui, n'en produit pourtant pas d'une ſeule qualité ; on y trouve la Malaquite ſoyeuſe, trop friable pour prendre le poli, et qui n'eſt que pour les cabinets ; la Malaquite fibreuſe et dure, laquelle prend ordinairement une couleur noirâtre au poli ; enfin la Malaquite lamelleuſe, quelquefois plus ou moins pale et mollaſſe, mais qui prend le plus beau poli et offre les plus belles nuances, lorſqu'elle eſt parfaite et dure.

" C'eſt de cette dernière eſpéce, et de la première qualité, que l' on offre aux amateurs riches un morceau qu'on peut dire unique et ineſtimable. Il eſt deja bien rare de trouver de cette qualité des morceaux aſſez grands et ſolides ; pour en tailler des plaques d'un demi-pied quarré. Le morceau, dont il eſt quéſtion, et dont le propriétaire de la mine (Mr de Tourtchaninof) lui-même proteſte n'avoir plus le ſemblable, avoit été preſenté par lui à un Seigneur de la Cour de Ruſſie, après la mort duquel il eſt tombé entre les mains du poſſeſſeur préſent. C'eſt une plaque taillée que deux hommes ont peine à ſoulever, de 26 pouces de

France de longueur, ſur 16 de large, et au-delà de deux d'épaiſſeur, de la plus belle couleur, d'une dureté parfaite, et preſque toute ſolide. Elle eſt actuellement depoſée à Londres dans le Cabinet de Mr le Preſident de la Societé Royale *Banks*, et les perſonnes qui deſirent de prendre à cet égard des informations, et de faire des offres, pourront s'adreſſer, franc de port, à Sr Joſeph Banks, Preſident de la Societé Royale, ſur Soho-ſquare à Londres, ou au propriétaire, Mr le Conſeiller de Cour Guthrie, Medecin du corps noble des Cadets de terre, et de celui des Ingenieurs à St Peterſbourg."

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By the channel of Dr Guthrie, we have alſo had communicated to us the following information to the public, reſpecting an important work which is at preſent ſet on foot in Ruſſia, by order of the Empreſs, and which is to be conducted by that learned and ingenious philoſopher Profeſſor Pallas.

"Avis au Public.——Les recherches ingenieuſes et profondes de pluſieurs ſavans de notre ſiécle, ſur l'affinité et l'origine des langues de nations, ſouvent très-éloignées entr'elles, et les éclairciſſemens de l'hiſtoire ancienne des hommes que pluſieurs hiſtoriens eſtimables ont ſû tirer de ces recherches, donnent aujourd'hui un nouvel attrait, une direction plus décidée, et un but plus philoſophique à une étude, laquelle juſqu'ici paroiſſoit ſêche, ingrate, et même ſterile et frivole à des eſprits ſuperficiels. En parcourant les ouvrages d'un Court de Gebelin, on reſte ſouvent étonné des inductions lumineuſes que l'auteur a ſû tirer de ce fond, et l'on ne peut s'empecher de régretter que cet homme laborieux n'ait pu ſoumettre à ſa methode toutes les langues de la terre. D'après l'analyſe et la comparaiſon heureuſe de celles qu'il avoit été à même de récueillir, perſonne ne doutera que la connoiſſance de celles que l'interieur de l'Aſie pouvoit lui fournir, ne l'auroit conduit à des découvertes encore bien plus intereſſantes.

" L'Empire de Ruſſie qui s'étend ſur une grande partie de cette Aſie, partie preſqu'inconnue aux ſavans dans les temps antérieurs à *Pierre le Grand*, contient ſans doute plus de nations et de peuplades, de langues et de dialectes, qu'aucun autre royaume de la terre. L'eſpace très reſſerré du Caucaſe, habité par des peuplades peu nombreuſes et très-voiſines entr'elles, recele plus de vingt et deux dialectes de huit ou neuf langues differentes. La Sibérie, plus vaſte, en offre un plus grand nombre encore, et la ſeule preſqu'isle du Kamtchatka, dont la population, lors de ſa découverte par les Ruſſes, ne ſembloit que commencée, contenoit neuf dialectes differens de trois langues héterogènes. La plûpart de ces langues ſont bien plus caracteriſées, ont bien moins de rapport entr'elles, et à toutes celles de l'Europe, que celles-ci ne'en ont conſervé avec l'ancien Celtique. Quel vaſte champ de découvertes, et quelle inſtruction pour l'hiſtoire, un litterateur judicieux ne pourra-t-il pas trouver dans une collection de cette grande varieté de langues de peuples, dont l'origine et les migrations nous ſont, pour la plûpart, abſolument inconnues, et dont les differentes tribus ſe trouvent ſouvent éloignées l'une de l'autre, à des diſtances immenſes, quelquefois en ſi petit nombre, que la langue court riſque de s'éteindre avec ces peuplades ?

Cependant la plûpart de ces langues eſt reſté juſqu'ici un tréſor caché pour les ſavans : on n'a pas même tenté de rapprocher ſur un plan uniforme, quelque nombre conſiderable de mots des langues déja connues. Les eſſais de quelques uns, de donner l'oraiſon dominicale, ou quelqu'autre ſuite de phraſes en differentes langues, ſont très-imparfaits, inſuffiſants, et n'ont rendu tout au plus qu'une centaine de langues et de dialectes, c'eſt à dire, le tiers à peu près de celles qui exiſtent. Pluſieurs litterateurs et hiſtoriographes ont comparé un petit nombre de langues anciennes ou modernes iſſues d'une ſouche commune. L'on trouve auſſi, outre la reſſource des dictionnaires, quelques vocabulaires iſolés et épars, ſouvent peu nombreux et rarement correſpondans, dans les voyageurs modernes. Mais perſonne juſqu'ici n'avoit embraſſé l'enſemble des langues que la diſperſion et les diviſions de l'eſpéce humaine, et l'influence des revolutions et des cauſes morales, phyſiques et politiques, pendant une longue ſuite de ſiécles et de generations, ont pu produire dans les terres habitables de tant de climats.

" Cette vaſte entrepriſe qui pourra enfin conduire à reſoudre le problême de l'exiſtence d'une langue primitive, étoit reſervée à notre ſiécle. *CATHERINE II.* a daigné ſe faire un délaſſement de cette partie encore inculte de la litterature. Pour ſervir de baſe à un gloſſaire univerſel et comparatif de toutes les langues, *Sa Majeſté Impériale* a fait Elle-même un choix des mots les plus eſſentiels et les plus generalement en uſage chez les peuples les moins cultivés. Son Empire ſeul pouvoit fournir pour ce gloſſaire, preſque le tiers de toutes les langues uſitées ſur le globe, et ſurtout un nombre conſiderable de ces langues encore ignorées des ſavans.

" Dans ce choix on a donné la preférence aux ſubſtantifs et adjectifs de première neceſſité, et communs aux langues les plus barbares, ou qui ſervent à tracer les progrès de l'agriculture, ou de quelques arts et connoiſſances élementaires d'un peuple à l'autre. Les pronoms, les adverbes, et quelques verbes, avec les mots numériques, dont la grande utilité pour la comparaiſon des langues eſt aſſez réconnue, y ont été admis, pour rendre ce gloſſaire plus complet et plus inſtructif.

" D'après cet excellent modelle l'on a recueilli d'abord toutes les langues et dialectes du vaſte Empire de Ruſſie; enſuite un nombre plus conſiderable encore de langues étrangères:

de ſorte que ce Recueil ſurpaſſe dejà, quoique continué ſeulement depuis l'année, tout ce qui a été tenté dans ce genre, et s'accroit encore continuellement par des materiaux de toute eſpéce.

" L'intention de *Sa Majeſté Impériale* eſt, que ce Recueil ſoit imprimé pour l'utilité du Public. Il ſera arrangé de façon, que chaque mot aura à ſa ſuite ſes traductions dans toutes les langues qu'il a été poſſible d'obtenir. Par ce moyen, et par une claſſification de ces traductions ſelon leurs rapports, l'affinité des langues deviendra plus apparente, et leur comparaiſon plus facile. La vraie prononciation des mots ſera exprimée avec la plus ſcrupuleuſe exactitude, par une orthographe uniforme et déterminée. Un tableau général des langues, tant ſelon leurs rapports que ſelon leur patries, pourra ſervir d'introduction à ce travail, dont les ſavans, particulièrement ceux qui peuvent en tirer parti, ne mèconnoitront pas la grandeur et la difficulté, et ſauront en apprècier le mérite.

" *Sa Majeſté Impériale* ayant bien voulu me nommer pour ſoigner la partie typographique de cet ouvrage juſqu'à preſent unique, je ne ſaurois aſſez tôt en avertir le Public, dont l'im-

patience égalera mon empreſſement à remplir les ordres diſtingués de ma *Souveraine.*

St Peterſbourg ce 22 de Mai, 1785.

P. S. Pallas."

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In the Journal de Medecine of Paris, for March 1785, we have the following liſt of prize-queſtions propoſed in the public aſſembly of the Royal Society of Medicine of Paris, held on the 15th of February 1785.

1. A prize of 600 livres, founded by the King, and which was firſt propoſed on the 26th of Auguſt 1783, for the following queſtion: "Déterminer quels ſont les avantages et les dangers du quinquina, adminiſtré dans le traitement des différentes eſpéces de fiévres remittentes?" Memoirs on this ſubject are directed to be ſent by the 1ſt of May 1785.

2. A prize of 360 livres, propoſed on the 31ſt of Auguſt 1784, on the following queſtion: "Déterminer quels avantages la médecine peut eſpérer des decouvertes modernes ſur l'art de reconnoitre la pureté de l'air, par les differens eudiomètres?" Memoirs on this ſubject

jećt are direćted to be ſent by the 1ſt of July 1785.

3. A prize of the value of 600 livres, propoſed on the 2d of March 1784, for the following queſtion: "Des quatre conſtitutions annuelles admiſes par les anciens, et qui ſont la catarrhale, l'inflammatoire, la bilieuſe, et l'alarbilieuſe; les trois premières etant connues, et bien determinées, on demande ſi la quatrième a une exiſtence diſtinćte, et quelle eſt ſon influence dans la produćtion des maladies epidémiques?" Memoirs upon this ſubjećt are direćted to be ſent by the 1ſt of January 1786.

4. A prize of 400 livres propoſed on the 31ſt of Auguſt 1784, for the following queſtion: "Déterminer quelles ſont, relativement à la temperature de la ſaiſon, et à la nature du climat, les précautions à prendre pour conſerver, après un campagne, la ſanté des troupes qui rentrent dans leurs quartiers, et pour prevenir les epidemies dont elles y ſont ordinairement attaquées?" Memoirs on this ſubjećt are direćted to be tranſmitted by the 1ſt of January 1786.

5. A prize of 600 livres, propoſed on the 31ſt of Auguſt 1784, for the following queſtion:

ſtion : "Déterminer par l'obſervation, quelle eſt la cauſe de la diſpoſition aux calculs, et autres affections analogues auxquelles les enfans ſont ſujets ; ſi cette diſpoſition depend des vices de l'oſſification, et quels ſont les moyens de la prevénir et d'en arrêter les progrès ?"

6. A prize of the value of 600 livres, propoſed on the 31ſt of Auguſt 1784, for the following queſtion : "Déterminer quels ſont les caractères des maladies nerveuſes, proprement dites, telles que l'hyſtericiſme, l'hypochondraciſme, *&c.* ; juſqu'à quel point elles different des maladies analogues, telles que la melancholie ; quelles ſont leurs cauſes principales ; et quelle méthode l'on doit employer en général dans leur traitement ?" Memoirs on this ſubject are directed to be tranſmitted by the 1ſt of January 1786.

7. A prize of 600 livres, propoſed firſt on the 11th of March 1783, and again on the 31ſt of Auguſt 1784, for the following queſtion : "Déterminer quels ſont les rapports qui exiſtent entre l'état du foi, et les maladies de la peau ; dans quels cas les vices de la bile, qui accompagnent ſouvent ces maladies, en ſont la cauſe, ou l'effet ; indiquer en même temps les

ſignes propres à faire connoitre l'influence des uns ſur les autres, et le traitement particulier que cette influence exige?" Memoirs on this ſubject are directed to be tranſmitted by the firſt of May 1786.

8. A prize of 600 livres, founded by the King, propoſed on the 15th of February 1785, for the following queſtion: "Déterminer, par l'examen comparé, des propriétés phyſiques et chymiques, la nature des laits de femme de vache, de chévre, d'aneſſe, de brebis, et de jument?" Memoirs on this ſubject muſt be ſent by the firſt of May 1786.

9. A prize of 600 livres, firſt propoſed on the 11th of March 1783, and afterwards on the 15th of February 1785, for the following queſtion; "Déterminer, 1*mo*, quelles ſont parmi les maladies, ſoit aigues, ſoit chroniques, celles qu'on doit regarder comme vraiment contagieuſes; par quels moyens chacune de ces maladies ſe communique d'un individu à un autre; 2*do*, quels ſont les procédés les plus ſurs pour arrêter les progrès de ces differentes contagions?" Memoirs on this ſubject are directed to be tranſmitted by the 1ſt of May 1787. For this prize, the Society are indebted to Mr

Lenoir, Counſellor of State, and Lieutenant General of the police in Paris. On this queſtion ſeveral memoirs were formerly given in, but moſt of them contained only foreign diſcuſſions, and were defective in obſervations. One, indeed, which had for its motto, "Les virus contagieux ne ſont point nés avec la nature," is repreſented as being entitled to much praiſe; but not having fulfilled the views of the Society in every particular, the queſtion is again propoſed. And they have ſtated, at ſome length, the objects which they wiſh to have aſcertained, reſpecting each of theſe particulars, included under it. To allow ſufficient time for a proper inveſtigation of each of theſe ſubjects, the delay till May 1787 has been thought neceſſary. And although the queſtion be again propoſed entire, yet thoſe who ſhall anſwer only one of its members in a ſatisfactory manner, ſo as to furniſh uſeful obſervations, will be honoured with a reward proportioned to the merit of their reſearches. And Mr Lenoir has authoriſed the Society to intimate that he will defray the expence. Memoirs on this, as well as on all the other queſtions, muſt be tranſmitted to

M.

M. Vicq d'Azyr, Secretary to the Royal Society of Medicine of Paris.

The Academy of Sciences and Belles Lettres at Dijon, have propoſed the following prize-queſtion for 1786: "Déterminer par leurs propriétés reſpectives, la difference eſſentielle du phlogiſtique, et de la matière de la chaleur?" Diſſertations on this ſubject muſt be tranſmitted to M. Maret, Secretary to the Society, by the 1ſt of April 1786.

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The Preſident and Council of the Royal Society of London, adjudged for the year 1784, the medal on Sir Godfrey Copley's donation, to Edward Waring, M. D. Lucaſian Profeſſor of the Mathematics at Cambridge, for his mathematical communications to the Society.

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In the Journal de Médecine for Auguſt, the Author announces the ſpeedy publication of a French tranſlation of Dr Cullen's firſt lines of the Practice of Medicine, under the following title: "Inſtitutions de médecine pratique, traduites ſur la quatrième et dernière edition

de l'ouvrage Anglois de M. Cullen, Profeſſeur de médecine pratique dans l'Univerſité d'Edinbourg, des Sociétés Royales de Londres, d'Edinbourg, *&c.* premier Médecin du Roi pour l'Ecoſſe, par M. Pinel, Docteur en Médecine."

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We mentioned in our laſt volume, that Mr Bell, engraver in Edinburgh, who, not long ſince, publiſhed an elegant edition of Dr Albinus's anatomical tables of the muſcles, and who has lately been honoured with the appointment of engraver to his Royal Highneſs the Prince of Wales, for his principality of Scotland, was engaged in an important anatomical work of plates of the blood-veſſels, nerves, brain, viſcera, parts of generation, *&c.* In this work he has made conſiderable progreſs. He has now engraved all the plates of Baron Haller's correct and elegant faſiculi, of the ſame ſize with the originals, excepting three, which are a little diminiſhed, to bring them to the ſize of the book. He has engraved alſo, ſuch of the blood-veſſels as ſeemed neceſſary, and which Dr Haller did not live to accompliſh, from Euſtachius, Walterus, *&c.* To Haller's figures of the eye, he has added all thoſe of Zinn, which make the repreſentation of the various parts of that organ moſt complete. He has done the lymphatics from Hewſon, and ſo much of the viſcera from Cheſelden, as anſwered his purpoſe. In ſome particulars, however, as the late Dr Hunter informed him, would be the caſe, he has found difficulty in collecting proper materials for executing what he had conceived in his mind. But from the elegant and ſplendid edition of Cowper's anatomy of the human body, reviſed and publiſhed at Leyden by Albinus, with Euſtachius, Monro, Walterus, *&c.* he hopes to be able to give a diſtinct exhibition of the viſcera, parts of generation, diſtribution of the nerves, *&c.*

The plates already executed amount to about ninety in number; the remaining plates, which will amount to about twenty more, he hopes may be finiſhed in leſs than twelve months.

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Dr Martin Wall has been appointed Clinical Profeſſor at the Radcliffe Infirmary in Oxford, and Dr William Thompſon has been appointed

Profeſſor of Anatomy in Chriſt-church College of that Univerſity, both vacant by the death of Dr Parſons.

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Dr Buſick Harwood has been appointed Profeſſor of Anatomy in the Univerſity of Cambridge, vacant by the death of Dr Colignon.

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During the courſe of the year 1785, the following gentlemen have been admitted members of the Royal College of Phyſicians in Edinburgh: Dr Andrew Farquharſon, and Dr Thomas Karr, on the 1ſt of February; Dr John Clark and Dr William Farquharſon, on the 3d of May; and Dr James Campbell and Dr Henry Maclagan, on the 1ſt of November.

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On the 8th of Auguſt 1785, Mr Robert Lawſon was admitted a member of the Royal College of Surgeons of Edinburgh.

About

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About the 1ſt of May, Dr Duncan will begin at Edinburgh, his lectures on the Materia Medica. This courſe, which is given only during the ſummer-ſeſſion, will continue for three months, and will be illuſtrated, not only by ſpecimens of the different articles, and of the different formulæ of the London and Edinburgh Pharmacopœias, but alſo by a variety of pharmaceutical experiments.

Before the commencement of this courſe, Dr Duncan propoſes to publiſh an improved edition of his catalogue of the Materia Medica, intended as a ſynopſis for his lectures on that ſubject. In the former editions of this liſt, the articles were arranged alphabetically. But beſides the officinal, the Linnæan, and the Engliſh name of each article, it contained alſo an account of the natural order to which it belonged, of the ſource from whence it could be moſt advantageouſly obtained, particularly, whether it ſhould be gathered in the fields, raiſed in gardens, or imported from abroad, of the different fixed formulæ of the Edinburgh Pharmacopœia into which it enters, and of the different extemporaneous formulæ under which it might be moſt conveniently uſed. To this liſt, which will be reprinted with a few alterations, he now means to add two other arrangements of the Materia Medica, one according to the natural orders to which the different articles belong, the other according to their obvious operation on the human body, or generally received medical virtues. But though theſe liſts will, in many caſes, give a diſtinct view of the affinities of different ſubſtances, with reſpect to medical powers, yet, in his lectures, he propoſes, as formerly, to treat of each article in alphabetical order, giving a full account of the natural and medical hiſtory of thoſe moſt frequently employed in modern practice. During the winter, Dr Duncan reads lectures on the Theory and on the Practice of Medicine, and on the moſt important caſes of patients ſubjected to chronical diſeaſes, treated at the Diſpenſary.

Pupils are admitted to attend Dr Duncan's lectures on the following terms:

1. For the lectures on the Theory of Medicine, One Guinea.
2. For the lectures on the Practice, Two Guineas.

3. For the lectures on the Materia Medica, Two Guineas.
4. For the caſe-lectures, One Guinea.

Thoſe who attend any of the three firſt mentioned courſes, are entitled to the caſe-lectures, for the payment of half a guinea at each courſe, as medicine-money. The fee for a perpetual pupil to all Dr Duncan's lectures is four guineas, excluſive of medicine-money when they attend the caſe-lectures, or five guineas including the caſe-lectures.

About the 1ſt of May, Dr Webſter will begin at Edinburgh his ſummer courſe of Chemical Lectures. This courſe continues for three months, and is repeated thrice every year, beginning in February, May, and November. The fee for each courſe is two guineas, and thoſe who attend it are alſo entitled to the caſe-lectures at the Diſpenſary, for the payment of the medicine-money only.

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About thirty years ago, a monument was erected in the Grayfriars church-yard at Edinburgh, to the memory of a young gentleman who had died during the courſe of his medical ſtudies at that place. It contained the following inſcription on a plain tablet of freeſtone:

"Here lyeth interred, John Barnet, ſtudent of phyſic, who was born on the 15th of March 1733, and departed this life on the 1ſt of April 1755.

Peace gentleſt ſhade; this from a brother's hand,
Who muſt not ſay what juſtice might demand;
Yet this I muſt, not each proud marble can,
The duſt beneath was truly once a man;
From virtue's pleaſing paths he never rov'd,
Of man a lover, and by man belov'd;
For others ills he griev'd, contemn'd his own;
To none ſevere, ſave to himſelf alone.
To him fair Science oped her uſeful page,
Rich with experience, and the ſpoils of age;
Too early loſt, lamented here he lies,
Nip'd like a roſe-bud, ere it blows it dies.
Death, leſt mankind he from the tomb ſhould ſave,
Snatch'd him thus early to the peaceful grave."

As this monument had, from different accidents, gone into ſuch diſrepair as to be in danger of falling down altogether; and as there was reaſon to preſume that the affectionate brother of this once promiſing young man, might be ſituated at a great diſtance from Edinburgh, the Æſculapian Society, who have given many proofs of their deſire of encouraging merit among the living, by perpetuating the remembrance of departed worth, made an application to the proper officer, the Dean of Guild of the city, for permiſſion to repair it. This is accordingly now done, with the addition of the following inſcription, on a freeze under the cornice:

> "Quiſquis hoc ſuſtulerit aut juſſerit,
> Ultimus ſuorum moriatur.
> Repaired by the Æſculapian Club. Edinburgh, 1785."

SECT.

SECT. IV.

Liſt of New Books.

THE Structure and Phyſiology of Fiſhes, explained and compared with thoſe of Man, and other Animals, illuſtrated with Figures. By Alexander Monro, M. D. Fellow of the Royal College of Phyſicians, and of the Royal Society, and Profeſſor of Phyſic, Anatomy, and Surgery, in the Univerſity of Edinburgh. Folio. Edinburgh.

Firſt Lines of the Practice of Phyſic. By William Cullen, M. D. Profeſſor of the Practice of Phyſic in the Univerſity of Edinburgh, Firſt Phyſician to his Majeſty for Scotland, Fellow of the Royal College of Phyſicians of Edinburgh,

burgh, of the Royal Societies of London and Edinburgh. A new Edition, corrected and enlarged, in 4 vols 8vo. Edinburgh.

Inſtitutions of Medicine; Part 1. Phyſiology for the uſe of Students in the Univerſity of Edinburgh. By William Cullen, M. D. Profeſſor of the Practice of Phyſic, *&c.* The third Edition, corrected. 8vo. Edinburgh.

A Treatiſe on the Influence of the Moon in Fevers. By Francis Balfour, M. D. Surgeon in the Service of the Honourable Eaſt India Company. Printed at Calcutta in 1784, and reprinted at Edinburgh by the deſire and recommendation of William Cullen, M. D. *&c.* 8vo. Edinburgh.

A Syſtem of Surgery. By Benjamin Bell, Member of the Royal Colleges of Surgeons of Ireland and Edinburgh, one of the Surgeons to the Royal Infirmary, and Fellow of the Royal Society of Edinburgh. Illuſtrated with Copperplates. Vol. 4. 8vo. Edinburgh.

A Treatiſe of Midwifery, comprehending the Management of Female Complaints, and Treatment of Children in early Infancy, for the Uſe of Female and other Practitioners, and private Families. By Alexander Hamilton, M. D.

Profeſſor of Midwifery in the Univerſity, Member of the Royal College of Surgeons, and Fellow of the Royal Society of Edinburgh. 8vo. Edinburgh.

Obſervations on the Animal Oeconomy, and on the Cauſes and Cure of Diſeaſes. By John Gardiner, M. D. Preſident of the Royal College of Phyſicians, and Fellow of the Royal Society of Edinburgh. 8vo. Edinburgh.

A Treatiſe of Midwifery, Theoretical and Practical, illuſtrated with Copperplates. By David Spence, M. D. Licentiate of the Royal College of Phyſicians, Edinburgh, and Fellow of the Society of Scottiſh Antiquaries. 8vo. Edinburgh.

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INDEX.

INDEX.

A.

Page.

ACADEMY of Sciences at Berlin, prize-queſtion for 1786, 350
—————— at Paris, prize-queſtion for 1786, 351
Acetum lithargyrites, remarks on, 234
Adair, Dr James, remarks on certain articles of the materia medica, 233
Aerial acid, deſcription of, 60
——— means of procuring, 61
——— means of combining with other matters, 64
——— in the atmoſphere, its influence in inducing diſorders, 89
Aeriform fluid diſcharged from the ſurface of the human body, 108
——— perſpiration, method of collecting, 111
Air-bag in fiſhes, its communication with the alimentary canal, 180
Air, obſervations on the changes it undergoes in the lungs, 97
Alkaline ſalts, their union with the aerial acid, 67
Alumen rupeum, remarks on, 233
Anaſarca, uſe of digitalis in, 143

Animal

Page.
Animal air, its properties, 113
——— œconomy, treatiſe on, 188
——— heat, obſervations on, 192
Animals, how affected by the aerial acid, 90
Apoplexy from the heat of the ſun, 208
Aſafœtida plant, deſcription of, 160
Æſculapian Society repair the monument of a ſtudent of medicine, 395

B.

Barnet, Mr John, account of his monument in the Grayfriars Church-yard at Edinburgh, 394
Barometer, cauſes of its variations, 52
Bell, Mr Andrew, account of his anatomical plates, 388
Bergman, Sir Torbern, phyſical eſſays, 56
Bilious intermitting fever, obſervations on, 201
Blood-letting in fevers, obſervations on, 203
Bordenave, M. eſſay on the neceſſity of the Cæſarian operation, 102
Brain of fiſhes furniſhed with lymphatics, 181
——— obſervations on, 183

C.

Cæſarian operation, obſervations on, 96
——— eſſay on the neceſſity of, 102
Calcination of magneſia, effects of, 77
Calx antimonii, in what different from Dr James's powder, 204
Campbell, Dr James, admitted a member of the College of Phyſicians of Edinburgh, 390
Cantharides,

Page.
Cantharides, internal uſe of, remarks on, 240
Catarrh, ſimple, obſervations on, 196
Catarrhal fever, obſervations on, 198
Catalepſis, hiſtory of a caſe of, 242
Cauſticity, its dependence on elective attraction, 75
Cauſes of fever, enumeration of, 197
Cheſt, effuſions in the cavity of, giving the appearance of diſeaſes of the liver, 116
Cholera morbus, obſervations on, 201
——— in the Eaſt Indies, account of, 209
Chorea ſancti viti cured by cuprum ammoniacum, 289
Circulation in fiſhes, obſervations on, 174
Clark, Dr John, elected Fellow of the College of Phyſicians in Edinburgh, 390
——————— remarks on his obſervations on ipecacuanha in dyſenteries, 213
Clouds, obſervations on, 49
Cloves raiſed in the botanical garden in the Iſle of France, 354
Cochrane, Dr Thomas, account of a remarkable protruſion of the inteſtines terminating favourably, 276
Cold, enquiry concerning its effects, 191
—— the cauſe of catarrhal fever, 197
—— produced by the mixture of vitriolic acid with ſnow, 367
Colic ſucceſsfully treated by tobacco glyſters, 127
Colignon, Dr Charles, account of, 332
Collingwood, Mr T. obſervations on the Peruvian bark, 265
Conſtipation, painful, caſes of, 255

Page.

Conſtitutions of patients, what favourable or unfavourable to digitalis, 141

Crawford, Dr, objections to his theory of animal heat, 192

Critical days, obſervations on, 200

Crows, account of their gaſtric fluid, 15

Cullen, Dr William, French tranſlation of his Firſt Lines of Practice, 388

Cuprum ammoniacum, its effects in the cure of chorea ſancti viti, 288

Cuttle-fiſh, obſervations on, 186

D.

Delirium from bile in the ſtomach, 203

Dick, Mr William, obſervations on dropſies in the Eaſt Indies, 207

Dick, Sir Alexander, account of, 334

Digeſtion, function of, experiments on, 3

——— organs of, their ſecreted liquors in fiſhes, 179

Digitalis, effects of large doſes of, 138

——— rules for the adminiſtration of, 139

Diſſection of a dropſical patient, 231

Diuretic power of digitalis, account of, 142

Dixon, Dr Joſhua, account of the appearances on the diſſection of a child dying of hydrocephalus, 312

Drink in dropſies, how to be uſed during the uſe of the digitalis, 140

Dropſy, caſes of, cured by foxglove, 135

Dropſical caſes ſucceſsfully treated by tobacco, 124

Dropſical

Page.

Dropſical caſes where foxglove was uſed, diſſection of, 360
Dropſies in the Eaſt Indies, obſervations on, 207
——— ſymptoms of, 216
Duncan, Dr A. medical lectures at Edinburgh, 391
Dyſentery, its connection with intermittent fever, 202
Dyſuria, caſes of, ſucceſsfully treated by tobacco, 126

E.

Eagles, account of their gaſtric fluid, 29
Ear, account of its anatomy in cetaceous fiſhes, 183
Echinus marinus, obſervations on, 186
Edinburgh, account of the aſafœtida plant growing in the botanical garden there, 161
Effervеſcence on the conjunction of alkalis with acids, its cauſe, 68
Elaterium, its uſe in dropſies, 220
Elaſtic vapours, their diſſolution in air, 46
Emmenagogues, remarks on, 339
Epilepſy, uſe of digitalis in, 143
Evans, Dr John, account of an uncommon ſwelling of the lower extremities, 302
——— hiſtory of a caſe of hydrocephalus, 299
Evaporation, theory of, 51
Eyes of fiſhes, obſervations on, 184

F.

Farquharſon, Dr Andrew, elected Fellow of the College of Phyſicians of Edinburgh, 390

Page.
Farquharſon, Dr William, elected Fellow of the College of Phyſicians at Edinburgh, 390
Fermentation in digeſtion, obſervations on, 36
——— eſſay on, propoſed by the Royal Medical Society of Edinburgh as the ſubject of their prize-queſtion for 1786, 348
Fevers, obſervations on, 195
Fire, its effect in evaporation, 50
—— the influence of aerial acid on, 89
Fiſhes, treatiſe on the ſtructure of, 165
Fitzpatrick, Dr, hiſtory of a caſe of catalepſis, 242
Fixed air, obſervations on the ſenſe in which the term has been taken, 59
——— proof of its being an acid, 64
——— remarks on, 236
Fowler, Dr Thomas, medical reports of the effects of tobacco, 122
Foxglove, account of its medicinal uſes, 133
——— ſucceſsfully employed in inſania and hæmoptyſis, 357
——— ſuſpected to have proved fatal in a dropſical caſe at Edinburgh, 358

G.

Gallinaceous birds, the influence of the gaſtric juice in their ſtomachs, 9
Gardner, Dr John, his obſervations on the animal œconomy, 188

Page.

Gas, animal experiments for aſcertaining its properties, 113
Gaſſeous air, how formed from vital air, 101
Gaſtric fluid, account of, 10
——— juice, obſervations on its acidity, 305
Gerard, Dr James, hiſtory of a caſe of ileus, 293
Glandular organs of fiſhes, remarks on, 177
Goettingen, faculty of medicine of, prize-queſtion for 1786, 349
Gonorrhœa, uſe of laudanum in, 153
Guthrie, Dr Matthew, ſends the aſafœtida plant to Edinburgh, 161
——————— experiments on the freezing of mercury, 368
——————— account of rabies canina ſucceſsfully treated by mercury, 369

H.

Hæmorrhage from the liver, obſervations on, 121
Hæmoptyſis cured by foxglove, 357
Hairs, ſelection of, for hygrometers, 40
Harveian ſociety, prize-diſſertations for 1784, 340
——————— prize-queſtion for 1786, 346
——————— their prize-queſtion for 1787, 347
Harwood, Dr Buſich, elected anatomical profeſſor at Cambridge, 390
Heat, diſeaſes reſulting from its exceſs, 200
—— enquiry concerning its effects, 191
Heart, power of the digitalis over its motion, 143

Page.
Hope, Dr John, his deſcription of the aſafœtida plant, 160
Human hair, hygrometer of, 38
Human ſpecies, experiments on digeſtion in their ſtomachs, 33
Hunter, Mr John, his practice in gonorrhœa, 153
Hydrocephalus, uſe of digitalis in, 144
——— uſe of mercury in, 356
——— account of the appearances on diſſection of a caſe of, 312
——— caſe of, terminating ſucceſsfully, 299
Hygrometer, eſſay on, 36
Hygrometrical obſervations, means of improving, 54
Hyoſcyamus niger, propoſed by the Harveian Society as the ſubject of their prize-queſtion for 1786, 346

I.

Ileus, hiſtory of a caſe of, 293
Imperforated anus, account of, 279
Infants apparently dead, means of recovering, 106
Infection, remarks on the way in which it has been ſuppoſed to enter the body, 196
Inflammable bodies, tendency of fixed air to unite with theſe, 81
Infuſion of tobacco, its diuretic power, 124
Inſania cured by foxglove, 357
Inſanity, uſe of digitalis in, 144
Intermiſſions, diſpoſition of marſh miaſmata to produce them, 202
Intermitting fever, obſervations on, 206

Page.

Inteſtines, remarkable protruſion of, terminating favourably, 276

Ipecacuanha, remarks on, 234

——— obſervations on, 341

——— its good in dyſenteria, 213

Irritability, ſubject of a prize-queſtion, 350

Irving, Dr Ralph, obtains the prize-medal of the Harveian Society for 1784, 340

Iron, how affected by the aerial acid, 79

James's powder, in what different from the calx antimonii, 204

K.

Karr, Dr Thomas, elected Fellow of the College of Phyſicians at Edinburgh, 390

L.

Lacteals in fiſhes, account of their courſe and termination, 181

Laudanum, its uſe in gonorrhœa, 153

Lavoiſier, M. experiments on the reſpiration of animals, 97

Lawſon, Mr Robert, admitted a member of the College of Surgeons of Edinburgh, 390

Leaves of digitalis, method of preparing them for medical purpoſes, 139

Lime, new burnt, cauſe of the heat it imparts to water, 74

Page.

Lithontriptics, propoſed by the Harveian Society as the ſubject of their prize-queſtion for 1787, 347

Living principle in animals, obſervations on, 188

——— remains in every part of the body ſome time after death, 190

Liver, account of the diſeaſes of, 115

Lower extremities, account of an uncommon ſwelling of, 302

Lungs, their appearance after death, occaſioned by the aerial acid, 91

Lymphatics in fiſhes, account of their courſe and termination, 181

Lyons, Dr, his uſe of opium in gonorrhœa, 154

M.

Maclachlan, Dr Alexander, account of the good effects of zinc in hyſteria, 245

Maclagan, Dr Henry, admitted a member of the College of Phyſicians of Edinburgh, 390

Macnab, Mr John, his account of exceſſive cold produced by vitriolic acid, 367

Magneſia, obſervations on, 76

Malachite, Ruſſian, account of, 373

Materia medica, catalogue of, by Dr Duncan, 391

Medical ſchool in Dublin, advertiſement concerning, 319

Mercury, experiments on the freezing of, 368

——— ſucceſsfully employed in rabies canina, 369

——— its effects in dropſies, 224

——— obſervations on its uſe in liver complaints, 214

Metals,

Page.
Metals, how affected by the aerial acid, 79
Meteorological obſervations, uſe of the hygrometer in, 54
Michaelis, Dr T. his trials of opium in ſyphilis, 148
Michel, J. P. de ſynchondrotomia pubis, 92
Mille, le Comte de, account of an aeriform fluid diſcharged from the ſurface of the human body, 108
Monro, Dr, his treatiſe on the phyſiology of fiſhes, 165
Mucous glands of the bronchiæ, their ſympathy with the ſkin, 197
Mucus of fiſhes, how ſecreted, 178
Muſcular action of the ſtomach, its influence, 5
Myers, Dr J. remarks on the Sigaultian operation, 281

N.

Natural hiſtory, diſſertations on, 1
Nerves, obſervations on, 190
——— obſervations on their aptitude in conducting the powers of action, 189
Nutmegs raiſed in the botanical garden in the Iſle of France, 355

O.

Oils, how affected by the want of fixed air, 82
Oliphant, Dr James, account of the preternatural adheſion of twins, 249
Opium, compariſon of its effects in ſyphilitic caſes with thoſe which take place in health, 150
——— method of exhibiting it as an antiſyphilitic medicine, 152

Page,

Opium, diſſertation on its uſe in ſyphilis, 146
——— explanation of the principles on which it operates in ſyphilis, 157
——— anſwers to objections urged to its uſe, 158
——— doubts reſpecting its uſe, 159
——— its uſe in phymoſis, 154
——— plaſter, its uſe in ſwelled teſticle, 155
——— propoſed by the Harveian Society, as the ſubject of their prize-diſſertation for 1785, 344
——— ſucceſs attending its uſe at Liſle in ſyphilitic caſes, 361
Organs of ſenſe in fiſhes, obſervations on, 183
Owls, account of their gaſtric fluid, 26

P.

Pallas, Dr, account of a work in which he is engaged, 376
——— raiſes the aſafœtida plant in Europe, 161
Paroxyſms, return of, in intermittents, how explained, 206
Parſons, Dr John, account of, 322
Pearſon, Dr George, experiment for determining the effects of bathing in Buxton water on perſpiration, 363
Perſpiration, aeriform, account of, 109
Perſpiration obſtructed during bathing in Buxton water, 364
Peruvian bark, obſervations on, 265
Phthiſis pulmonalis, uſe of digitalis in, 145
Phymoſis, uſe of opium in, 154
Portal, M. account of diſeaſes of the liver, 115

Page.

Prize-queſtions by the Royal Medical Society of Paris, 382
———— of Edinburgh, 346
Profeſſorſhips of medicine in Dublin, eſtabliſhment of, 319
Pubis, ſection of the ſymphyſis, treatiſe on, 92

R.

Rancidity of oils, cauſe of, 82
Regimen during the uſe of opium in ſyphilis, 152
Reſpiration of animals, experiments on, 97
Root of digitalis, its uſe in medicine, 136
Royal Medical Society of Edinburgh, their prize-queſtion for 1786, 347
Royal Medical Society of Paris, their prize-queſtions, 382
Rules reſpecting the employment of tobacco, 129

S.

Sal catharticus amarus, remarks on, 235
Saline purgatives, their effects in dropſy, 225
Sauſſure, M. de, eſſay on the hygrometer, 36
Saunders, Dr Richard, account of, 325
Sea-egg, obſervations on, 186
Sedative power of opium, proof of, 149
Secretion in fiſhes, obſervations on, 177
Senſe, organs of, in fiſhes, obſervations on, 183
Sepia loligo, account of, 186
Sheep, experiments in digeſtion in their ſtomachs, 22
Skin, its ſympathy with mucous glands the cauſe of catarrh, 197

Page.

Sickneſs from the digitalis, how to be counteracted, 141

Sigaultian operation, remarks on, 281

———— deſcription of the method of performing, 283

Simple elective attractions of fixed air, table of, 85

Skate-fiſh, communication between its pericardium and the cavity of the abdomen, 178

Smoke, how attracted by the aerial acid, 90

Spallanzani, Abbé, diſſertations on natural hiſtory, 1

Stimuli, obſervations on, 190

Stomachs, diviſion of, 3

———— muſcular, experiments on, *ib.*

———— membranous, experiments on, 19

———— intermediate, experiments on, 12

Stomach, morbid fluids in, neceſſary for the cold ſtage of fever, 199

———— reaſon for believing that infection enters the body by means of it, 196

Sympathy, obſervations on, 190

Syphilis, account of the uſe of opium in, 146

T.

Teeth, tranſplantation of, diſeaſe induced by, 362

Theuſink, J. A. Thomas, account of the uſe of opium in ſyphilis, 146

Thompſon, Dr, elected anatomical Profeſſor at Oxford, 390

Tobacco-glyſters, obſervations on their uſe in colic, 127

Tobacco, medical reports of the effects of, 123

Truth, rules for the inveſtigation of, in chemical enquiries, 57

Page.

Twins, preternatural adheſion of, 249
Tympanites, remarks on, 356

U.

Ulcers, venereal, ſucceſsfully treated by opium, 155

V.

Venereal buboes ſucceſsfully treated by opium, 155
Veſicular vapours, obſervations on, 48
Vinegar, its influence in diminiſhing the emetic power of ipecacuanha, 343
Vital air, queſtion whether abſorbed at the lungs, 102
Vitriolic acid, its influence in producing exceſſive cold, 367
Vitriolum cœruleum, remarks on, 238
Volatile alkali, its conjunction with the aerial acid, 72
Vomiting from obſtructions of the liver, 120
——— obſervations on its occurrence in dropſies, 217

W.

Wall, Dr Martin, elected chemical Profeſſor at Oxford, 389
Walker, Dr Joſhua, his obſervations on the uſe of cuprum ammoniacum, 288
Waring, Dr Edward, obtains the prize-medal of the Royal Society for 1784, 387
Warren, Dr John, caſes of painful conſtipation, 255
Water, means of uniting it with the aerial acid, 65
Webſter, Dr Charles, chemical lectures at Edinburgh, 393

Page.

Weather at Edinburgh, journal of, 353

White, Mr Charles, remark on his account of ſwelling of the lower extremities, 304

Withering, Dr William, account of the medicinal uſes of foxglove, 133

Y.

Yellowneſs, how far a ſymptom of diſeaſes of the liver, 118

Z.

Zinc, good effects of, in hyſteria, 247

—— how affected by aerated water, 80

General

General Alphabetical TABLE of CONTENTS of the Sixth, Seventh, Eighth, Ninth, and Tenth Volumes of the Medical Commentaries.

N. B. *The Roman numbers mark the Volume, and the Arabic characters mark the page.*

A.

AASKOW, V. B. Obſervatio de febre putrida petechiali, a fœtore piſcium ſalitorum putrefactorum, vol. vi. p. 248.

Abilgaard, P. C. Tentamina electrica in animalibus inſtituta, particula prima, vi. 252.

Adair, Dr James, Hints on particular articles of the materia medica, ix. 206.

——————— Additional remarks on certain articles of the materia medica, x. 233.

Aery, Dr Thomas, Hiſtory of a caſe of hydrocephalus ſucceſsfully treated by mercury, viii. 332.

Akerman,

Akerman, Dr John, Treatiſe on the knowledge and cure of the triſmus or locked jaw, vi. 386.

Alix. Matth. Franc. Obſervata chirurgica, vi. 295.

——————— Obſervata chirurgica. Faſciculus primus, vi. 154.

Armſtrong, Dr F. Account of ſingular convulſive fits in three children of one family, ix. 317.

Atchiſon, Mr Robert, Obſervations on the dyſentery among the negroes on the coaſt of Guinea, ix. 268.

Athil. Dr Sam. Byam. De aquæ frigidæ uſu externo, vi. 62.

B.

Balfour, Francis, M. D. Treatiſe on the influence of the moon in fevers, ix. 147.

Baſſignot, M. Hiſtoire de la maladie connue ſous le nom de crinons, ix. 64.

Bentely, Emmanuel, M. D. Diſſertatio inauguralis anatomica obſtetricia de ſectione ſynchondroſeos oſſium pubis, vii. 272.

Bergmann, Sir Torbern, Phyſical and chemical eſſays, vol. 1.; x. 56.

Biſſet, Dr Charles, Obſervations on lymphatic encyſted tumours, ix. 244.

Black, William, M. D. Obſervations, medical and political, on the advantages and diſadvantages of general inoculation, eſpecially in cities, viii. 141.

Blackhall, Dr Andrew, account of, viii. 113.

Bland, Mr Thomas, account of the effects of cuprum ammoniacum in cure of epilepſy, vii. 240.

Bohadſch, Joannes, M. D. Diſſertatio de utilitate electrizationis in arte medica, viii. 7.

Bordenave, M. Memoire ſur la néceſſité de faire l' operation Cæſarienne aux femmes qui meurent encientes, x. 102.

Borthwick, Mr George, Account of the ſucceſsful operation of the trepan on the left temple, with the extraction of a ſplinter of ſtone, penetrating the dura mater, viii. 322.

———————— Hiſtory of a ſingular caſe of delirium from a wound of the head, vii. 439.

Bowen, Mr James, Account of a ſingular tumour in the groin, removed by extirpation, ix. 233.

Boyer, M. Traitement d'une affection ſoporeuſe, vi. 418.

Brougton, Dr Arthur, Hiſtory of two caſes of dropſy, ix. 368.

Brugmans, Antonius, Magnetiſmus, ſeu de affinitatibus magneticis obſervationes academicæ, viii. 202.

Bucquet, M. Mémoire ſur la maniere dont les animaux ſont affectées par differens fluides aeriformes mephitiques, et ſur le moyens de remedier aux effets de ces fluides, ix. 70.

———— Obſervations ſur l'analyſe de l'opium, ix. 84.

C.

Cairncroſs, Mr Andrew, Hiſtory of a curious caſe of a remarkable and ſurpriſing recovery of a private ſoldier, who had his cranium ſo fractured as to require the trepan; a compound fracture of the lower extremity requiring amputation; and ſeveral other wounds, viii. 296.

Calderwood, Mr Robert, account of a diſcharge of animals by the anus, much reſembling the common caterpillar, and which were found to be the larva of an inſect, ix. 223.

Campbell, Dr A. Account of the ſucceſsful treatment of hydrocephalus by mercurialis, ix. 240.

Campbell, Mr Ivie, Account of a ſewing-needle lodged in the breaſt of a woman, removed by extirpation, ix. 275.

Camper, Pet. M. D. & P. Les avantages de l'inoculation, & la meilleure methode de l'adminiſtrer, viii. 130.

Chavaſſe, Mr William, Hiſtory of a caſe of tetanus, ſucceſsfully treated by opium, ix. 374.

Clarke, Dr John, Hiſtory of a caſe of an obſtructed ſecretion of urine, vi. 204.

Clarke, Dr John, Obſervations on fevers, eſpecially thoſe of the continued type; and on the angina maligna, vii. 150.

Cochrane, Dr Thomas, Hiſtory of a large wound in the abdomen, with a remarkable protruſion of the inteſtines, terminating favourably, x. 276.

Colignon, Dr Charles, Account of, x. 332.

Collingwood, Mr Thomas, Account of an uncommon diſcharge from an opening made into a large tumour in the under part of the belly and back, ix. 344.

——————— Obſervations on the Peruvian bark, x. 265.

Crawford Adair, M. D. Experiments and obſervations on animal heat, and the inflammation of combuſtible bodies, vi. 399.

Cruttwell, Mr C. Advice to lying-in women, vi. 258.

Cullen, William, M. D. and P. Firſt lines of the practice of phyſic, vol. ii.; vii. 284.

——————— Firſt lines of the practice of phyſic, vols iii. and iv.; ix. 19.

Curtin, Dr Samuel, Obſervations on the yellow fever of the Weſt Indies, ix. 236.

D.

Darbey, Mr, Account of the good effects from the vapour-bath in an hydropic caſe, ix. 305.

Darwin, Mr Charles, Account of, vi. 227.

Darwin, Mr Charles, Account of the retrograde motions of the abſorbent veſſels of animal bodies in ſome diſeaſes, vii. 193.

Deaſe, Mr William, Obſervations on the different methods of treating the venereal diſeaſe, viii. 25.

Delius, Henr. Fred. Diſſertatio de Taraxaco, viii. 35.

Simmons, Sam. Foart. M. D. Obſervations on the cure of the gonorrhœa, viii. 42.

Dick, Mr William, Obſervations on dropſies prevailing among the troops in the Eaſt Indies, x. 207.

Dick, Sir Alexander, Account of, x. 334.

Dixon, Dr Joſhua, Account of appearances on the diſſection of a child dying of hydrocephalus, x. 312.

——————— Hiſtory of a caſe of angina polypoſa, ix. 254.

Dobſon, Dr Matthew, Account of his method of treating the dropſy of the brain by mercury, vi. 224.

——————— Medical commentary on fixed air, vii. 31.

Dougall, Mr William, ſurgeon, Hiſtory of a caſe of ileus, in which a conſiderable portion of the inteſtine was voided by ſtool, ix. 278.

Drummond, Dr, A. M. Account of, viii. 420.

Duguid Leſlie, Dr P. Account of, viii. 425.

Dumonceau, M. Obſervations ſur le contre-poiſon du ſublime corroſif, vi. 414.

Duncan, Dr A. Account of his hypotheſis reſpecting the cauſe of animal heat, vi. 98.

——— Medical caſes, with remarks and obſervations, vi. 300.

——— Account of his medical lectures at Edinburgh, x. 391.

E.

Eaſon, Dr Alexander, Hiſtory of a caſe of hydrocephalus, ſucceſsfully treated by the uſe of mercury, viii. 325.

Ebeling, Jo. Theod. Phil. Chriſt. Diſſertatio medica inauguralis de Quaſia, et Lichene iſlandico, vi. 367.

Elliot, Dr J. Philoſophical obſervations on the ſenſes of viſion and hearing, vii. 76.

Evans, Dr John, Hiſtory of a caſe of retroverted uterus, vi. 215.

Evans, Dr John, Hiſtory of an obſtinate affection of the bowels, cured by an injection of a decoction of tobacco, vi. 332.

——— Hiſtory of a caſe of hydrocephalus terminating ſucceſsfully, x. 299.

——— Hiſtory of an uncommon ſwelling of the lower extremities in a pregnant woman, x. 302.

Fitzgerald, Dr Samuel, Hiſtories of the diſcharge of a large calcareous concretion, and the extraction of the bones of a fœtus by the rectum, viii. 329.

Fitzpatrick, Dr J. Account of extraordinary effects from the application of cold water after delivery, ix. 227.

——— Hiſtory of a caſe of catalepſis ſucceſsfully treated, x. 242.

Forbes, Mr Daniel, Hiſtory of a caſe of ileus, where a bliſter was of great benefit, ix. 266.

Forkenbeck, Frid. Diſſertatio inquirens cauſam perfectæ depletionis vaſorum in cadavere detectæ, vii. 54.

Fowler, Dr Thomas, Obſervations and experiments on the effects of different anthelmintics applied to earth-worms, viii. 336.

Fowler, Dr Thomas, Hiſtory of a caſe of rheumatiſm, cured by the volatile elixir of guaiac, vii. 94.

——————— Hiſtory of a remarkable caſe of the morbid effects of lightning ſucceſsfully treated, vi. 194.

——————— Medical reports of the effects of tobacco, x. 122.

Fynney, Fielding-Beſt, Eſq; Hiſtory of an uncommon caſe in midwifery, accompanied with a luxation of the maxilla inferior, occaſioned by convulſions, ix. 380.

G.

Gardner, John, M. D. Obſervations on the animal œconomy, and on the cauſes and cure of diſeaſes, x. 188.

Gaubius, Dr H. Account of, viii. 111.

Gerard, Dr James, Hiſtory of a caſe of ileus terminating fatally, x. 293.

——————— Hiſtory of a ſpeedy recovery after the operation of trepan, ix. 272.

Gourlay, William, M. D. Hiſtory of a caſe of encyſted ſarcocele, ix. 336.

Grieve, Dr John, Hiſtory of a caſe of inveterate dropſy, ſucceſsfully treated by combining cathartics and diuretics, ix. 286.

——— Mr William, Obſervations on the uſe of the bark of the angeline-tree as an anthelmintic, ix. 365.

Guerick, Jo. Ern. Theoph. Diſſertatio de ſulphure antimonii aurato liquido, vii. 58.

Gulbrand, Joh. Wil. M. D. de ſanguifluxu uterino, vi. 21.

——————— de pareſi metaſtatica, brachii ſineſtri a gonorrhœa, vi. 256.

Guthrie, Dr Matthew. Obſervations on the plague quarentines, &c. viii. 345.

——————— Obſervations on the uſe of mercury in rabies canina, x. 367.

H.

Haffner, Georg. Diſſertatio medico-chirurgica de hydrope articulorum, vi. 132.

Haighton, Mr J. Hiſtory of two caſes of fractured olecranon, with remarks, ix. 382.

Hall, Mr Charles, Account of a new ſpecies of palſy, vi. 71.

Hamilton, Dr Alexander, Treatiſe of midwifery, comprehending the whole management of female complaints, and the treatment of children in early infancy, viii. 52.

——— Dr Robert, of Lynn Regis, Account of a ſucceſsful method of treating inflammatory diſeaſes by mercury and opium, ix. 191.

——— ——— of Ipſwich, Hiſtory of a caſe of angina pectoris, by which it would appear that the complaint is ſometimes hereditary, ix. 307.

——— ——— History of an obſtinate caſe of epilepſy ſucceſsfully treated by bloodletting, vii. 336.

Havnienſis ſocietatis medicæ collectanea, vi. 247.

Henry, Thomas, F. R. S. Method of ſweetening or preſerving water at ſea; alſo a mode of impregnating large quantities of water with fixed air aboard ſhips, and in hoſpitals; to which is added, a proceſs for preparing artificial yeaſt, viii. 63.

Heyſham, John, M. D. Account of the jail-fever, or typhus carcerum, viii. 190.

——— History of a remarkable caſe of epilepſy, and dyſphagia ſpaſmodica, cured by by the uſe of cuprum ammoniacum, vii. 428.

Heyſham, John, M. D. Hiſtory of a caſe of epilepſy cured by cuprum ammoniacum, vii. 438.

Hey, William, F. R. S. Obſervations on the blood, vi. 376.

Home, Francis, M. D. and P. Clinical experiments, hiſtories, and diſſections, viii. 205.

Hope, John, M. D. Deſcription of a plant yielding aſafœtida, x. 160.

Houlſton, Dr, Directions for preventing fatal effects from drinking large quantities of ſpirits, viii. 354.

——— Obſervations on canine madneſs, viii. 304.

——— Obſervations on mineral poiſons, vi. 325.

Huck Saunders, Dr Richard, account of, x. 325.

Hunter, John, Eſq; A practical treatiſe on the diſeaſes of the teeth, vi. 180.

Hunter, Dr William, Account of, viii. 426.

Huſſey Garret, M. D. A phyſical enquiry into the cauſe and cure of fevers, vii. 182.

Ingen-

I.

Ingen-Houſz, John, M. D. Experiments on vegetables, diſcovering their great power of purifying the common air in ſun-ſhine, and injuring it in the dark and at night, vii. 391.

Irvine, Dr J. Account of a very intenſe degree of cold obſerved at Glaſgow, vii. 120.

J.

Jackſon, Dr Sequin Henry, Hiſtory of a ſingular affection of reſpiration, with an account of the appearances on diſſection, vi. 208.

———————— Treatiſe on ſympathy, in two parts, viii. 150.

Jaſſay, Jo. Diſputatio inauguralis medica, ſiſtens tentamina quædam cum aere fixo in ægrotis inſtituta, vii. 301.

Jones, Mr Thomas, Hiſtory of a caſe of a flap-operation, united by the firſt intention, ix. 326.

Johnſon, Cuthbert, M. D. Hiſtory of a dropſy of the ovarium terminating fatally; with an account of the appearances on diſſection, vii. 91.

Johnſtone, Edward, M. D. Hiſtory of a puerpural fever; with remarks on the treatment of that affection, vii. 98.

———— James, M. D. Hiſtory of a caſe of paralyſis rheumatica, cured by tinct. guaiac. volat. and the application of cauſtics, ix. 388.

———————— Treatiſe on the malignant angina, or ulcerous ſore throat, vi. 267.

K.

Kerr, Mr William, Account of the operation of amputating the thigh at the upper articulation, vi. 337.

Kilgour, Mr Thomas, Hiſtory of a caſe, in which worms in the noſe, productive of alarming ſymptoms, were ſucceſsfully removed by the uſe of tobacco, viii. 75.

Kirkland, Thomas, M. D. Thoughts on amputation; to which is added, a ſhort eſſay on the uſe of opium in mortifications, vii. 302.

Knox, Dr William, Hiſtory of a caſe in which cataracts in both eyes were removed by electricity, ix. 303.

Koſtrewſki, Jacobus, Diſſertatio medica inauguralis de gratiola, vi. 141.

Lanphier,

L.

Lanphier, Sim. Diſſertatio medica inauguralis de calculo renum et veſicæ, vi. 50.

———— Dr S. Hiſtory of a caſe of rheumatiſm cured by electricity, viii. 314.

Laſſone, M. de, Hiſtoire de divers accidens graves, occaſionnés par les miaſmes d'animaux en putrefaction, ix. 57.

Lavoiſier, M. Experiences ſur la reſpiration des animaux, et ſur les changemens qui arrivent à l'air en paſſant par leur poumon, x. 97.

Leith, Dr Charles, Hiſtory of an uncommon convulſive cough cured by flowers of zinc, vi. 343.

Le Roy, M. Alphonſe, Recherches hiſtoriques et pratiques, ſur la ſection de la ſymphyſe du pubis, vi. 39.

———— Du prognoſtic dans les maladies aigues, vi. 7.

Lieutaud, Dr Joſeph, Account of, viii. 112.

M.

Macbride, Dr David, Account of, vii.

Maccaufland, Mr Robert, Facts and obſervations on different medical ſubjects, viii. 247.

Maccormick, Dr Samuel, Hiſtory of a diabetes ſucceſsfully treated by the uſe of Dover's powder, ix. 349.

Mackie, Mr John, Caſe of the ſucceſsful treatment of hydrocephalus internus, by mercury, vii. 221.

Maclauchlan, Alexander, M. D. Account of the good effects obtained from the calx of zinc, in an hyſterical affection, x. 247.

——————— Hiſtory of a ſingular caſe of purulent aſcites cured by tapping, ix. 360.

Mahon, Viſcount, Principles of electricity, vii. 396.

Wortmuller, Carolus-Henricus, Diſſertatio botanico-medica de catechu, vii. 412.

Maitland, Dr Robert, Hiſtory of an uncommon caſe in delivery, vi. 85.

Makittrick-Adair, James, M. D. Obſervations on regimen and preparation under inoculation, and on the treatment of the natural ſmall

ſmall-pox in the Weſt Indies, with ſtrictures on the Suttonian practice, viii. 211.

Marcard, Henry Matth. M. D. Mediciniſche verſuche, or, Medical eſſays, vii. 130.

——————— Medical eſſays, part 2. vi. 279.

Martineau, Mr Philip, Hiſtory of an uncommon enlargement of the abdomen from an affection of the kidneys, ix. 282.

Mertens, Car de, Obſervationes medicæ de febribus putridis, de peſte, nonnulliſque aliis morbis, vi. 162.

Milly, M. le Comte de, Mémoire ſur une ſubſtance aeriforme, qui emane du corps humain, et ſur la maniere de la recueillir, x. 108.

Michaelis, Chriſt. Fred. Diſſertatio inauguralis de angina polypoſa ſive membranacea, vii. 13.

Michell, Jan. Peterſen, M. D. De ſynchondrotomia pubis commentarius, x. 92.

Monro, Alexander, M. D. and P. Obſervations on the ſtructure and functions of the nervous ſyſtem, ix. 1.

——————— The ſtructure and phyſiology of fiſhes explained, x. 165.

Mudge,

Mudge, John, M. D. Radical method of curing a recent catarrhous cough, vii. 383.

Myers, Dr J. Remarks on the Sigaultian operation, x. 381.

N.

Nachtigal, Jo. Winces, Diſſertatio de ſubmerſis, viii. 162.

Niven, Mr David, Hiſtory of a caſe of imperforated hymen, cured by inciſion, iv. 330.

O.

Obſervations on the gaſtric juice, by ——— x. 305.

Oliphant, James, M. D. Account of an uncommon caſe in midwifery, where a preternatural adheſion of twins had taken place. x. 249.

Orred, Mr Daniel, Succeſsful method of cure in diſeaſes of the larger joints, which have been hitherto thought to require amputation, vii. 325.

P.

Parſons, Dr John, Account of, x. 322.

Pattiſſon, Mr J. Account of, viii. 410.

Paxton, Mr Richard, Obſervations on opening the thorax and abdomen of a young lady, who died after a very ſhort illneſs, viii. 90.

Pearſon, George, M. D. Obſervations and experiments for inveſtigating the chemical hiſtory of the tepid ſprings of Buxton, ix. 124.

Percival, Thomas, M. D. F. R. S. Philoſophical, medical, and experimental eſſays, viii. iii. vi. 31.

——————— Remarks on Mr Daniel Orred's treatment of diſeaſes of the larger joints, vii. 231.

Pharmacopœia Roſſica, viii. 307.

Portal, M. Memoire ſur quelques maladies du foie qu'on attribue à d'autres organes, x. 115.

Pott, Percival, F. R. S. Remarks on that kind of palſy of the lower limbs, which frequently accompanies a curvature of the ſpine, vi. 318.

Price, James, M. D. F. R. S. Experiments on mercury, ſilver, and gold, viii. 176.

Pringle, Sir John, Account of, viii. 416.

Probſt. Jo. Franc. Ignat. Diſſertatio medica inauguralis de ſale volatili cantharidum, vi. 151.

Prochaſka, Georgius, M. D. Diſſertatio de Urinis, vi. 17.

R.

Radniczky, Ignat. Specimen inaugurale medicum, ſiſtens experimenta quædam de ſenſibilitate, viii. 16.

Rait, Mr William, Account of the good effects of Peruvian bark and Madeira wine, in an obſtinate ulcer of the leg, ix. 354.

Regnaudot, Jo. Maria, Diſſertatio medica inauguralis de chirurgia infuſoria renovanda, vii. 46.

Remmet, Dr Robert Butler, Hiſtory of a caſe of hydrocephalus, vi. 422.

Ring, Mr John, Account of a new method of treating dropſy, viii. 83.

Ryan, Dennis, M. D. Hiſtory of a caſe where the pharynx was wounded through the muſcles and membranes which connect the larynx and os hyoides, without proving fatal, viii. 319.

Ryan, Dennis, M. D. Account of, viii. 420.

S

Saillant, M. Recherches ſur la maladie convulſive epidemique, attribuée par quelques obſervateurs à l'ergot, et confondue avec la gangrene ſeche de Solignots, ix. 73.

Sandifort, Ed. M. D. et P. Obſervationes anatomico-pathologicæ, vii. 178.

Sartine, M. de, Reglemens concernant la propreté des vaiſſeaux, et la conſervation des equipages, vii. 260.

Saunders, William, M. D. Obſervations on the ſuperior efficacy of the red Peruvian bark, in the cure of agues, and other fevers, viii. 167.

Sauſſure, Horace Benedict de, Eſſais ſur l'hygrométrie, x. 36.

Scot, Dr William, Hiſtory of a caſe of aſcites, remarkable for the quantity of water drawn off by tapping, vi. 440.

Schwediauer, Fr. M. D. Practical obſervations on the moſt obſtinate and inveterate venereal complaints, ix. 90.

Sherwin, Mr John, Hiſtory of the cure of a dangerous obſtruction of the trachea, where Mr Mudge's inhaler was uſed with advantage, vii. 416.

Sibbern, Dr, Obſervatio de tænia, ope ſtanni raſi expulſa, vi. 251.

Simmons, Samuel Foart, M. D. Practical obſervations on the treatment of conſumptions, vii. 63.

Spallanzani, Abbé, Diſſertations relative to the natural hiſtory of animals and vegetables, x. 1.

Speer, Dr William, Account of the advantageous effects derived from the cortex ſemiroubæ, in an obſtinate fluor albus, vi. 443.

———— Hiſtory of an uncommon caſe in midwifery, vi. 443.

Solander, Dr D. C. Account of, viii. 418.

Steidele, Jo. Obſervationes de rupto in partus doloribus utero, vi. 123.

Storer, Dr John, Obſervations on the uſe of the cuprum ammoniacum in ſpaſmodic affections of the abdominal viſcera, and in hyſteria, vii. 229.

Stuart, Dr David, Hiſtory of a caſe of the pemphigus major of M. Sauvages, vi. 79.

Swan, Dr, Hiſtory of a caſe of retroverted uterus, vi. 217.

T.

Theuſſink, Ev. Jo. Thom. Diſſertatio de opii uſu in ſiphylide, x. 146.

Thomas, Mr Thomas, Hiſtory of a caſe affording a proof of the power of extravaſated blood in diſſolving bone, vi. 75.

Tilton, James, M. D. Hiſtory of a ſingular caſe of rabies canina terminating favourably, vi. 429.

Tralles, Balth. Lud. De uſu veſicantium in febribus acutis, vi. 262.

W.

Wall, John, M. D. Medical tracts, vii. 377.

Warren, Dr John, Hiſtory of two caſes of painful conſtipation from indurated fæces, x. 225.

Walker, Dr Joſhua, Obſervations on the uſe of cuprum ammoniacum in chorea ſancti Viti, x. 288.

Willan, Robert, M. D. Hiſtory of a caſe of hydrocephalus, with the appearances on diſſection, vii. 333.

Wilmer, B. Eſq; Caſes and remarks in ſurgery, with the method of curing the bronchocele, vii. 22.

Withering, William, M. D. An account of the foxglove, and ſome of its medicinal uſes, x. 133.

——————— Account of the ſcarlet fever and ſore throat, or ſcarlatina anginoſa, vi. 279.

Y.

Young, Dr Thomas, Account of, viii. 423.